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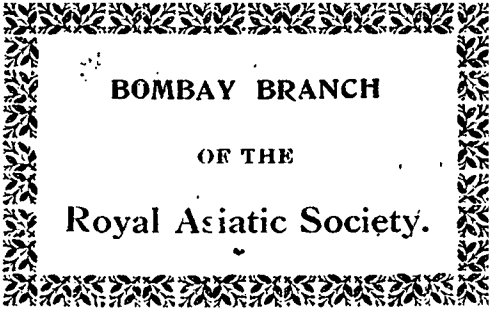

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GRIMM'S LAW

A STUDY

OR

HINTS TOWARDS AN EXPLANATION

OF THE SO-CALLED

"LAUTVERSCHIEBUNG"

TO WHICH ARE ADDED

SOME REMARKS

ON THE

PRIMITIVE INDO-EUROPEAN *K*

AND

SEVERAL APPENDICES.

66583

T. LE MARCHANT DOUSE.

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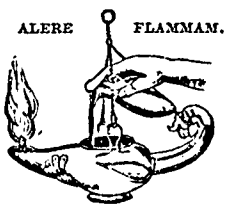

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PREFACE.

THE subject of this little Treatise came unavoidably in my way almost on the threshold of a somewhat more extensive investigation upon which I had proposed to myself to enter. Among other preliminary inquiries, it was necessary for my immediate purpose to see whether certain groups of I-E. roots, the members of each of which seem to offer the marks of a close relationship to one another, could, with any approach to certainty, be severally traced to a single parent form; and this inquiry, as it advanced, became at length inextricably involved with another arising out of a very obvious fact, namely, that the varieties of form exhibited by *all* the members of a group within *one* of the great dialects correlated by Grimm's Law are identical with the varieties of *one* member of a group as exhibited by *all* the great dialects so correlated. Looking about for the various explanations of this fact, I found it scarcely possible not to surmise that all those varieties of form, whether collected in one dialect or dispersed among several, must have originated by phonetic variation in the same way and at the same time, and that their distribution in each and among all of the aforesaid dialects must have

been due to the relationship that subsisted among those dialects in primeval times. The pursuit of these and such-like conjectures led me farther and farther astray from the line of inquiry originally marked out; and being (like most people who have thought on the matter) quite dissatisfied with the current hypotheses of Grimm's Law, I resolved at last to detach this subordinate subject altogether, and see what conclusions respecting it were attainable by a rigid application, to the facts before me, of linguistic principles that were either already demonstrated, or that appeared to me demonstrable.

I did this the more willingly because the limited time at my disposal for any studies of the kind seemed more likely to suffice for a monograph upon a single and definite problem than for an investigation into larger questions. It soon became manifest, however, that to elaborate all the points of interest that offered themselves at almost every step of even my more limited way would tax the powers of an inquirer who could devote his whole time to the work. Hence, although I have found myself able to consider a few of the more important of such points in separate Appendices, yet as regards many others I have been compelled to rest content with mere hints or bare statements, in the hope of resuming the study of them at a future time.

But neither inevitable shortcomings of this kind, nor some other defects both in form and in matter, of which I am sufficiently conscious, will very greatly grieve me, if I shall only have succeeded in clearly

working out the principal features of the theory hereinafter propounded. Looked at in the most general and comprehensive way, the characteristic of this theory will be seen to lie in its treatment of Grimm's Law as a compound, instead of a simple, phonetic problem, combining in itself (not of any natural necessity, but through a unique conjuncture of linguistic conditions) two distinct problems; of which one involves the *origin* or *generation*, and the other the *distribution* or *arrangement*, of the sounds composing the several Mute-systems covered by the Law. It will be found, I hope and believe, that this method of treatment, whatever else it may do or leave undone, will avoid the most glaring objections to the prevalent hypotheses on the subject. For, on the one hand, by the origin assigned to the weaker Mutes, the evolution of the German Mute-systems, instead of appearing to invalidate the almost universal Principle of Debilitation (or of "Least Effort," § 6), is actually accounted for by that principle; while, on the other hand, in favour of the process to which the symmetrical arrangement of the Mutes in the related systems is attributed it becomes possible to produce *some* evidence, at any rate, from similar phonetic processes actually observable.

What is here attempted, therefore (to borrow an illustration from a sister science), closely resembles, in a small way, what has been achieved in Astronomy on a grand scale; for as it was only when the orbital motions of the planets were no longer regarded as simple and uncompounded, but were treated each as

the resultant of two rectilinear motions, that both they themselves came to be properly understood, and that the planetary masses were shown to be as obedient as all other matter to the universal sway of attraction; so, in the present case, I have ventured to think that Grimm's Law, by the analysis here proposed, will become susceptible, as a whole, of a satisfactory explanation, and will, in particular, be reduced to subjection under the Principle of Least Effort,—as, indeed, it ought to be. For this Principle is, in Phonology, exactly what Gravitation is in the system of the Universe; and no case of sound-change can with perfect safety be represented as in direct opposition to it, unless counteracting agencies can be actually detected at work. Finding nowhere the slightest ground for thinking that any such agencies ever contributed to the phenomena of Grimm's Law,—finding everywhere, in fact, good grounds for thinking the reverse,—I have deemed it of prime importance to reconcile the Law with the aforesaid Principle; and to effect this reconciliation is the first, and, while really the more important, not perhaps the more difficult, of the two problems which go to make up the compound one.

But the most striking and interesting, although at the same time the most perplexing, aspect of Grimm's Law lies in that distribution of its implicated sounds which will here furnish the subsidiary component problem. It is this that differentiates it from other cases of sound-change; and by this it stands unique. There are not wanting, indeed, other and simpler cases of symmetrical distribution (§§ 18, 39); but they are

only approximations to this case; and the reasoning from them to it must needs be constructive only. In the treatment of this problem accordingly we shall very likely have to venture on untrodden ground and to enunciate new principles; and if it shall appear that the evidence adduced to establish these principles is never so copious, and sometimes not so clear, as could be wished, let it be remembered that such a drawback is inseparable from the necessarily narrow sphere of observation of any individual inquirer. If the attention of other observers, over wider linguistic areas, shall ever be directed to a search for more and better evidence of a similar kind, I cannot but think that much will be forthcoming.

The mention of evidence leads me to remark that the later sections of this Essay (unless it be the last of all), which treat of the phenomena exhibited by the I-E. *k*, are not to be dissociated from the body of the book. Those phenomena, it is true, are not without great interest of their own; but this fact would of itself by no means justify an examination of them here. They are introduced ostensibly as being directly explicable by the principles previously laid down in the book; but they may really be looked upon as offering another and a very important example (albeit on a less extensive scale) of the very same modes of phonetic evolution and distribution as those exemplified by Grimm's Law itself, and therefore as a valuable additional support to the line of reasoning by which an explanation of the Law has been attempted.

I beg leave to state, in conclusion, that in em-

ploying the title of "*Grimm's* Law" I do not intend to express an opinion in favour of the great German's right of paternity in the Law. My own feeling is rather one of regret that Rask's prior claims are not more generally acknowledged. For this is a case in which the Hesiodic paradox holds good, that the half is more than the whole; in other words, it was a greater achievement on Rask's part to demonstrate, in the first instance, the relationship between the Classical and L.G. Mutes, than on Grimm's part to elaborate and extend the demonstration afterwards. But I am afraid it is now too late to make a change. The title I have adopted has taken possession of the ear of the world of letters, and will not very easily be superseded.

London, July 1876. THE AUTHOR.

ABBREVIATIONS.

A-S.	= Anglo-Saxon.	L.G.	= Low German.
Cl.	= Classical.	M.H.G.	= Middle High German.
C.G.	= Comparative Grammar.	N.H.G.	= New „ „
Comp.	= Compendium.	O.H.G.	= Old „ „
D.	= Deutsch -e, -er, &c.	O.N.	= Old Norse (Icelandic).
D.G.	= Deutsche Grammatik.	O.Sl.	= Old (Ecclesiastical) Slavonic.
Gesch.	= Geschichte.		
G.C.	= Grammaire Comparée.	Spr.	= Sprache.
H.G.	= High German.	V.G.	= Vergleichende Grammatik.
I-E.	= Indo-European.	W-B.	= Wörterbuch.
Li-Sl.	= Lithu-Slavonic.	Z-S.	= Zeitschrift.

The Titles of Books are mostly abbreviated; but none of the references will offer any difficulty, unless it be "Whitney (Jolly)," which is put for the almost interminable title of Dr. Jolly's German adaptation of Prof. Whitney's well known Lectures.

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GRIMM'S LAW: A STUDY.

1.—NEXT in point of philological importance to the revelation of Sanskrit to the scholars of Europe, Curtius justly places the discovery of that phonetic relationship between certain Indo-European languages whose expression in set terms is known as "Grimm's Law." For not only did this discovery at once enable men to demonstrate, with almost mathematical precision, the real identity underlying the apparent differences in the vocabularies of those languages; but it led the way to the investigation of the whole body of Indo-European articulate sounds; the systematized results of which investigation, under the name of Phonology or Sound-Lore, serve in turn as an indispensable basis for the entire superstructure of Comparative Grammar. Nor is this all; for while, on the one hand, Grimm's Law has proved for the philologist a potent instrument of Scientific Inquiry, on the other hand the concise formulation of which it is susceptible, and the case with which it may be applied on a small scale, have rendered it perhaps the most popular and widely known of all linguistic inductions. It supplies matter now-a-days for a section in many of our more pretentious English Grammars. It is even becoming a choice article in the regular stock-in-trade of Examiners; and boys and girls in their teens are sometimes required not merely *to* state, but to *explain* the Law in question; that is, either to show how the phenomena which it summarizes are reconcilable with known linguistic processes, or else to establish some hitherto unob-

served principle to which they ought to be referred. Now this is the very thing that is wanting; for, in the opinion of competent judges, the construction of a satisfactory theory of Grimm's Law is a problem which has long been, and still is, awaiting solution[1]. But as none of the many original investigations called into being by such requirements have, so far as I am aware, ever been made public, it is possible there may yet be room for the few hints which I here intend to offer.

2.—(*a*) This widespread knowledge of the leading *facts* involved in the problem before us, be it deep or be it shallow, is so far to the advantage of an inquirer after *principles* as to allow him to dispense with a detailed statement of those facts[2]. In this place, therefore, and simply as a provision for future reference, I shall only formulate the Law as concisely as possible. For such formulation, as well as mnemonically, no nomenclature is, in our language, so convenient as that which describes the three main classes of Mute-consonants as *Hard*, (*k, t, p,*) *Soft* (*g, d, b,*) and *Aspirate* (*kh, th, ph,* or *gh, dh, bh*). It is quite immaterial what objection may be urged against these designations on abstract or physiological grounds. They will be employed here as symbolic rather than descriptive. Indeed, I shall for the most part put aside the complete words, and, taking merely their initials, **H, S, A,** manipulate these pretty much as if they were algebraical symbols,—without, however, precluding myself from having re-

[1] "Die Lautverschiebung ist ohne Frage die bedeutsamste lautliche Thatsache in der Geschichte des Sprachenkreises in den unsere Muttersprache hineingehört, und bildet zugleich eine der allermerkwürdigsten und schwierigsten Erscheinungen ihrer Art, deren Erklärung die Sprachwissenschaft zu unternehmen hat. *Auch ist bis jetzt noch keine befriedigende Erklärung dafür gefunden.*"—Whitney (Jolly), p. 155.

"One of the most important problems, which now awaits solution, is to explain the causes of that regular *shifting* of sounds which words undergo in different cognate languages."—Sayce: "Principles of Philology," p. 47.

[2] Such a statement will be found in Baudry's "*Grammaire Comparée*," pp. 131-150; or Max Müller's Lectures, Second Series, Lect. v.: and a more concise summary in Morris's "Historical Accidence," Chapter ii.; or Earle's "Philology of the English Tongue," § 1.

course to other symbols or nomenclature if occasion should require it.

(*b*) Now the phenomena summarized in Grimm's Law stand out most strikingly upon a comparison of three principal Mute-systems. One of these, shared in (with certain known variations) by Sanskrit, Zend, Greek, and Latin, may for brevity be called the *Classical* (Cl.) system. The second, of which Gothic is generally taken as the best representative, but in which the Old Norse, Old Saxon, Anglo-Saxon, and other dialects shared, may be called the *Low German* (L.G.) system. The third, as characterizing High German tribes, may be called the *High German* (H.G.) or (inasmuch as it approached more nearly to complete regularity a thousand years ago than it does now) the *Old High German* (O.H.G.) system. Other Mute-systems, as for example the *Lithu-Slavonic* (Li-Sl.) and the *Keltic*, which partake of the characteristics both of the first and the second, are really not less important than these; but for the present they may be left out of sight. As regards the three first-named systems, Grimm's Law requires that a given mute in any one shall be represented in each of the others by a different mute of the *same family*, whether guttural, labial, or dental, as the case may be. Thus—

Classical **H** = Low German **A** = High German **S**;
„ **A** = „ „ **S** = „ „ **H**;
„ **S** = „ „ **H** = „ „ **A**;

or, grouping each system[1] horizontally,—

If the Classical system is **HAS** or **ASH** or **SHA**,
the corresponding L.G. is **ASH** or **SHA** or **HAS**,
the corresponding H.G. is **SHA** or **HAS** or **ASH**.

(*c*) That these three tabulations are of identical value, or severally represent precisely the same set of facts (differing only in order of sequence), will be seen by comparing the vertical columns of any one with those of any other. It is consequently quite indifferent which of them be taken to symbolize

[1] In order to avoid turning aside into explanations and qualifications, I assume for the present that all the three systems are ideally perfect. Their deviations from regularity will be considered at the proper time.

the Law. For our own convenience, however, it is desirable to select some one as a standard for future reference: let us therefore take the first. This done, a glance will detect the remarkable symmetry which characterizes the relationship between the three systems. As the eye passes down the lines of initials, each succeeding system appears to be derived from the foregoing one by precisely the same amount and precisely the same kind of change: in other words, whatever phonetic operation, as it were, is executed upon the Cl. system to produce the L.G. system, must also be executed upon the L.G. to produce the H.G. system; and (what is equally important, but is rarely if ever made prominent) the very same operation, when executed upon the H.G. system, brings us round again to the Cl. system. That is, descending to the individual sounds, if we pass (in the horizontal lines) from **H** to **A** and from **A** to **S** in any one system, such transition uniformly requires a corresponding transition from **A** to **S** and from **S** to **H** in the representative sounds of the following system. And a similar phenomenon presents itself if we read the horizontal lines from the lowest upwards; or, again, if we read them alternately (*i. e.* from Cl. to H.G., from L.G. to Cl., and from H.G. to L.G.). Thus, take them in what order we may, each system regularly varies with each of the others; so that, to borrow the language of Mathematics, each system may be called a *function* of each of the others[1].

(*d*) This will allow us to represent the systems and their relationship with much greater brevity than can be done even by the above tabulations. For as each system is made up of the same phonetic quantities, so to say, as the others (*viz.* **H**, **A**, and **S**), although in a different order, this general agreement may be indicated by using one and the same symbol (say $\Sigma = \Sigma\acute{\upsilon}\sigma\tau\eta\mu\alpha$) for them all; while their variation by sequence, and undetermined priority, may be indicated by the letters x, y, z. Putting therefore Σ_x, Σ_y, Σ_z, for the three sys-

[1] The analogy is perfect: compare Todhunter's Definition:—"Suppose two magnitudes which are susceptible of change so connected that if we alter one of them, there is a consequent alteration in the other, this second magnitude is called a *function* of the first." ("Diff. Calc." *init.*)

tems, we see by the relationships just now described, that Σ_x (whichever it may be) is a "function" both of Σ_y and Σ_z; Σ_y both of Σ_x and Σ_z; and Σ_z both of Σ_x and Σ_y. Thus, taking the systems in succession, and putting f for the function which Σ_x is of Σ_y, we have the series of equations—

$$\Sigma_x = f(\Sigma_y);\ \Sigma_y = f(\Sigma_z);\ \Sigma_z = f(\Sigma_x); \quad \ldots \quad (\alpha)$$

which leads to a repetition of the whole series. Again, *alternando*, putting ϕ for the function which Σ_x is of Σ_z we similarly have—

$$\Sigma_x = \phi(\Sigma_z);\ \Sigma_y = \phi(\Sigma_x);\ \Sigma_z = \phi(\Sigma_y); \quad \ldots \quad (\beta)$$

which also leads to a repetition of the whole series. In both cases therefore we have not merely functions, but functions which run round, as it were, in a circle.

(*e*) A comparison of these results with the tabulations in (*b*) above will make three facts plain:—(1) That the foregoing equations express all the possible relationships between the systems; for as these are three in number, they cannot be coupled in more than $3 \times (3-1)$ or six ways; (2) that as those six ways may be arranged in three correlated pairs (*e. g.*, $\Sigma_x = f(\Sigma_y)$, $\Sigma_y = \phi(\Sigma_x)$, and so on), the second series of equations does not really give a new value of any one system in terms of any other, but merely represents a back-reading from Σ_y to Σ_x, &c.; so that, instead of "*alternando*," we may equally well read "*invertendo*;" (3) that, as far as phonetic relationship is concerned, it is perfectly immaterial, and only a subject for arbitrary convention, which system shall be called the first; for Σ_x is evolved from Σ_z by precisely the same amount and kind of variation as Σ_z from Σ_y and Σ_y from Σ_x. Consequently the following single tabulation might be substituted for all the three tabulations in subsection (*b*) above, of which indeed it is but another reading:—

Cl. or L.G. or H.G. system = **H A S** = Σ_x;
L.G. or H.G. or Cl. „ = **A S H** = Σ_y;
H.G. or Cl. or L.G. „ = **S H A** = Σ_z.

And, lastly, by fact (2) it is immaterial whether the series of equations marked (α) or that marked (β) be taken to represent the relationship between the three systems.

3.—The principal point brought out in the preceding section is what may be called the Cyclo-functional relationship between Σ_x, Σ_y, and Σ_z. Applying to this the Principle of Sufficient Reason, we arrive at the conclusion that no one of the three systems so related may, in preference to either of the others, be assumed as the normal and primitive system, from which the others are only deviations; for it is manifest that no reason can be urged for or against the priority of one system which may not be urged with exactly equal force for or against the priority of each of the others. The only alternative is, as Max Müller says, that "none was before or after the other"[1], to which may be added that "none is greater or less than another:" that is, in short, none is anterior to the others in time or superior in importance. This negative conclusion stated in positive terms means that the evolution of Σ_x, Σ_y, and Σ_z, was the result of simultaneous or contemporaneous phonetic action; and I shall hereafter show that that action, as between each system and each or both of the others, must also have been mutual and reciprocal. Hence the symbol Σ_x (as was shown in § 2 (*e*)) belongs as much to one system as to another; and so do similarly Σ_y and Σ_z: so that the symbols we have selected are in danger of losing their value through vagueness of application. In order, therefore, that they may be conveniently employed, it is desirable to arrange them arbitrarily in some definite order. Let us then agree that Σ_x shall denote the Cl., Σ_y the L.G., and Σ_z the H.G. system: no harm will be done, provided we bear in mind all along that *x*, *y*, *z* indicate, not a necessary, but merely a conventional order.

4.—By the conclusion reached in the foregoing section we are brought face to face, in point-blank opposition, with the prevalent hypothesis of Grimm's Law. This may be called the Historical or Chronological Hypothesis. It was laid down by Grimm himself, and has been adopted with slight variations by Curtius and most of the leading Continental Philologists.

[1] Lect. v., Second Series; where, however, the statement has too much the aspect of a mere dictum.

It supposes that the Cl. system (our Σ_x) was the only primitive and original phonetic system, shared in by the ancestors of all the Indo-European nations alike; that the L.G. system (Σ_y) was, in point of time, a subsequent deviation from Σ_x; and the H.G. (Σ_z) a subsequent and precisely similar deviation from Σ_y. It necessarily leaves completely out of sight the functional relationship between Σ_z and Σ_x (*viz.* $\Sigma_z = f(\Sigma_x)$); for to suppose that Σ_x in its turn was a subsequent and similar deviation from Σ_z would obviously be such a glaring *reductio ad absurdum* as would at once overturn the whole hypothesis. But as the cyclo-functional relationship between the systems is a patent fact, the said hypothesis is driven to assume a successive series of phonetic move ments of such a kind that that relationship is accidentally caused to *simulate* contemporaneous and reciprocal action. It cannot, however, be, that the phonetic movements under consideration were simultaneous and yet successive—reciprocally or mutually related, and yet totally independent. Of propositions thus irreconcilably hostile one or the other must give way. But before we, on our side, relinquish in favour of the Chronological Hypothesis the conclusion of § 3 which was laid down as our starting-point, it may be worth while to inquire whether the agreement of that hypothesis with ascertained linguistic principles and the internal harmony of its own leading features are so complete as to justify our surrender. Such an inquiry will, at any rate, have the advantage of directing attention, not only to the difficulties involved in our main problem, but also to the fundamental processes in accordance with which a solution of that problem ought to be effected.

5.—(*a*) We have to remember, at the outset, that Grimm's Law does not express any primary and independent principle of language, but merely a particular case of Sound-change. The case, as regards its form, is certainly very complex. In this respect it may even be, and as far as we know is, quite unique; but its nature nevertheless remains unaltered. It should therefore take its place in the same rank as other

cases of Sound-change. It should be subjected to the wider law[1] or principle under which they are generally reducible. And the first thing we should expect to find in any Theory of Grimm's Law would be an attempt, at any rate, to harmonize the two.

(*b*) The wider law or principle just referred to is that which, following French writers on the subject, I shall call the "Principle of Least Effort." The sway of this principle really extends far beyond the realm of language; for it is but another name for the general tendency of mankind to make *all* labour as easy to themselves as possible. In its linguistic application it has been so well put by Professor Whitney that, to avoid stating it worse, I will venture on a somewhat hackneyed quotation. "All articulate sounds," says he, "are produced by effort,—by expenditure of muscular energy in the lungs, throat, and mouth: and this effort, like every other which man makes, he has an instinctive disposition to seek relief from, to avoid"[2]. This tendency, or "instinctive disposition," is as universal and continuous in its operation

[1] The word "law," whose associations used to be only noble, has been so hackneyed of late by writers on philological and other topics, in order to give factitious dignity to all sorts of trifling rules, that one hardly recognizes an old friend when occupying a position of importance.

[2] I quote the following version for the sake of Dr. Jolly's concurrence:—"Die erste und wichtigste Erscheinung....die Ursache fast aller Lautveränderungen....ist jener Trieb des Menschen....das Streben, den Sprachorganen die Sache leicht zu machen,—die schwerer sprechbaren Laute und Lautverbindungen durch bequemere, weichere zu ersetzen, und allen unnützen Ballast in den Wörtern über Bord zu werfen. Alle articulirten Laute werden mit einer gewissen körperlichen Anstrengung hervorgebracht, indem dabei die Muskelthätigkeit unserer Lunge, unserer Kehle, und unseres Mundes, in Anspruch genommen wird. Diese, gerade wie jede andere Anstrengung, sucht sich der Mensch, kraft eines natürlichen Instincts, vom Halse zu schaffen, oder doch zu erleichtern: eines Instincts, den man nach Belieben als einen Ausfluss der angeborenen Trägheit oder der Sparsamkeit, d. h. des Selbsterhaltungstriebes des Menschen betrachten mag; er fliesst in der That bald aus der ersteren, bald aus der letzteren Quelle, je nach den Umständen; er ist Trägheit, wenn dadurch mehr verloren als gewonnen wird,—weise Sparsamkeit wenn der Gewinn die Einbusse übersteigt. *Kein Tüttelchen, kein Jota*

as the tendency of water to run down hill. To it may be referred nearly all the phonetic changes which have taken place in the Indo-European languages within historic times; and as there is no reason to suppose that human nature (or, to be safe, let us say, our variety of it) was ever different in the main from what it still is, we may fairly conclude that the same principle began to operate as soon as there was any linguistic material to operate upon.

6.—(*a*) An important division of Phonology, therefore, is that which concerns itself with the relative ease and difficulty (where these are ascertainable) of both vowels and consonants; and which demonstrates that, apart from counteracting causes, the course of phonetic change uniformly proceeds from the harder to the easier sound. Thus Bopp, Corssen, and others make it an important preliminary to their investigations to adjust the weight or strength of the primitive (or classical) vowels; and to show that while the heaviest or strongest, *a* (=English *ah*), may descend to *u* (=*oo*) and *i* (=*ee*), neither *i* nor *u* ever reascends (*sua sponte*) to *a*. From this and similar considerations Grimm[1], Leo Meyer, and others have hazarded the bold conjecture that, at a very remote period in the history of the parent Indo-European Speech, *a* was the only vowel; and Fick (Wörterb., *Nachwort* I.) has attempted with considerable success to raise that conjecture to certainty[2].

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kann sich dem Einfluss dieses Triebs entziehen, und er kommt auf die mannigfachste Weise zum Durchbruch."—Whitney (Jolly), pp. 105–6.

So too M. Baudry:—"En résumé, comme il arrive pour tout acte humain, le langage livré à lui-même tend à s'exercer avec la moindre action; ou, ce qui revient au même, avec l'action la plus commode possible."—G. C., p. 85.

I add but one more statement:—"Alle Veränderungen der Laute, die im Verlaufe des sprachlichen Lebens eintritt, ist zunächst und unmittelbar Folge des Strebens, unseren Sprachorganen die Sache leicht zu machen; Bequemlichkeit der Aussprache, Ersparung an Muskelthätigkeit ist das hier wirkende Agens."—Schleicher: Die D. Spr., 3rd ed., p. 50.

[1] "Wiederum ist von den drei vocalen *A* der edelste, gleichsam die mutter aller laute, aus dem zunächst *I* und *U* hervorgegangen sind."—Gesch. der D. Spr., p. 274 (ed. 1848).

[2] He states (somewhat too unhesitatingly perhaps) the results of his

(*b*) In the case of the Mute-consonants the strongest family are the gutturals; next come the dentals; and lastly the labials. Gutturals, therefore, by the same principle may descend to dentals or labials; but, as a rule, neither dentals nor labials may reascend to gutturals. The line of debilitation for the different families of mutes, and mostly for the vowels, is mainly determined by the simple physical rule, that sounds produced in the *fauces* require the greatest muscular effort, and that such effort diminishes as the sounds are produced further and further along the vocal passage towards and up to the lips.

(*c*) But it is with the members of the separate families that we are now more particularly concerned; and here, inasmuch as all the members of each family are necessarily produced at or about one and the same point of the vocal passage, a different rule must hold good. The comparative strength of these is determined partly by the *energy* and partly by the *completeness* of the contact of the vocal organs at the point of production. The difference between the Hard and Soft mutes (Tenues and Mediæ) resolves itself roughly into the greater or less energy or muscular tension[1] with which they are produced; so that their relative strength is well enough indicated by their names. The Aspirates seem to have differed originally from the Pure (*i. e.* unaspirated) mutes by giving way very slightly—and, except to a fine ear, perhaps imperceptibly—to the *vis a tergo* supplied by the puff of breath which, in the utterance of the Pure mutes, was completely arrested[2]. Among many of the Indo-European peoples this aspiration soon advanced to a very perceptible interruption,

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investigations as follows:—"Sämmtliche Wurzeln auf *i* sind jüngere Formen von solchen auf *a*"; and again, "Ebensowenig wie *i* ist *u* ursprünglich, sondern durchweg aus *a*, in einigen Fällen auch aus *va*, entstanden." Hence in explaining Vowel-strengthening he postulates a time "als der *a*-Vocal *noch allein herrschte*."—W-B., pp. 950, 951, 956.

[1] "Czermak, by using his probe, found that hard consonants drove it up much more violently than soft consonants."—Max Müller: Lect. iii., Second Series.

[2] See Appendix B.

or rather prevention, of complete contact; and the result, under conditions of uniform development, at last yielded the three spirants *f*, *th*, and *h*, which are reckoned among the feebler sounds,—the last, indeed, being in the lowest stage of debilitation.

(*d*) The Holethnic[1] language is generally thought to have possessed, like the Greek, only *one* aspirate of each family,—whether a hard or soft aspirate has been vehemently contested[2]: a complete family, however, should possess *two*,—both a hard (**A**) and a soft (**A'**). These four, arranged in pairs according to their energy, stand thus: , ,— , **A'**; and, according to their completeness of contact, thus: **H**, **A**—**S**, **A'**; **A** and **S** therefore may be considered physiologically as debilitations by different agencies, and on opposite sides, so to say, of **H**; they start from it on divergent lines of descent; they are naturally deducible from it alone, and not from each other; and, in fact, like dissimilar quantities, they are hardly comparable. **A'**, however, may be regarded as producible in two ways,—either by the softening of **A**, or by evolution from **S** in precisely the same way that **A** is evolved from **H**. In representations of the primitive alphabet the order generally given is **H**, **A**, **S**; an order which seems to imply that the primitive Aspirate was the *hard* Aspirate. That it really was so I have no doubt: the inconsistency lies in this, that in the prevalent opinion it was the *soft* Aspirate. But it is not now essential for us to discuss more minutely the relative strength of the mutes of each family: enough for the present if it be admitted that both **A** and **S**, or at any rate and especially the latter, are weaker than **H**.

7.—If we test the Chronological Hypothesis of Grimm's Law by this wide-ruling Principle of Least Effort, we shall

[1] I shall venture, for brevity, to call the primitive undivided I-E. people the "Holethnos" (τὸ ὅλον ἔθνος = *gens integra*); whence the adjective "Holethnic" by correct derivation. The term "Proethnic", sometimes employed, besides being destitute of a related noun (for to talk of a "Proethnos" would be nonsense), implies that the primitive people were not themselves an *ethnos* or *gens*.

[2] See § 34 *infrà*.

find at each supposed phonetic evolution a glaring example of disobedience to the latter on the part of the former. For the **S** of Σ_x[1] is represented by **H** in Σ_y, and again the **S** of Σ_y by the **H** of Σ_z; that is, every Cl. Media must, by the hypothesis, have become a L.G. Tenuis, and every L.G. Media an O.H.G. Tenuis. Of the total number of mutes involved in the Cl. vocabularies, between 20 and 30 per cent.[2] are Mediæ; and these, by the hypothesis, must have become Tenues during the evolution of Σ_y. The L.G. mutes exhibit pretty nearly the same percentage of Mediæ (supposed to be derived from the Cl. Aspiratæ); and these in turn must have become Tenues during the evolution of Σ_z: so that, altogether, about half the total number of mutes which form the common property of the I-E. peoples must, by the same hypothesis, have undergone, at one stage or the other, a change directly opposite to that required by the Principle of Least Effort.

8.—(*a*) This incompatibility of the Chronological Hypothesis with the principle just named has not escaped the keen perception of the many able men who have adopted that hypothesis; and it becomes interesting to discover how they propose to remove it. Here and there a straightforward writer finds no other method than that of impeaching the universal action of the debilitating principle itself,—a method which merely amounts to a confession that his hypothesis cannot be brought into harmony therewith. There is besides a prevalent but very loose way of talking of the successive substitutions as if they were the result of the inherent nature, or of a necessary tendency, of the individual mutes of each family. Several remarks of Grimm himself seem to countenance this notion[3]; so also do the descriptions and "schema" of Schleicher[4]. It is scarcely to be wondered at, therefore, that the retailers of

[1] See the final tabulation of § 2 (*e*).

[2] More exact statistics will be quoted presently (§ 14).

[3] "In der media liegt gleichsam seine natürliche kraft [*i.e.*, that of the *Lautverschiebung*], die sich zur tenuis verdünnt und hernach wieder zur aspirata verdickt: aus der aspirata muss darauf die einfache media abtropfen, und *dann der umlauf neu beginnen.*"—Gesch., p. 416 (*et sic alibi*).

[4] Die D. Spr., p. 97.

oracular utterances speak with greater boldness on the subject [1].

(*b*) All this is, in fact, only the virtual assumption of a principle precisely the opposite of the Principle of Least Effort. It is as if one should declare that, because water is found at different altitudes, therefore half the rivers of the world must once have run up hill. Under special circumstances, it is true, and on a comparatively small scale, water may be urged above its ordinary level,—as, for example, by force of wind, or by mechanical means; and similarly may sounds be "raised" by some extraneous influence, such as the force of accent or emphasis or the "attraction" of a neighbouring sound [2]. But in the latter case, no less than in the former, we look about for an influential agent; and if none is at first discoverable, it is hardly consistent with scientific caution to declare that none exists. The leading supporters of the Chronological Hypothesis have evidently felt this, but have not always been content to wait for a suitable explanation. Indeed, the too hasty attempts to grasp at any means for propping up this weak side of the hypothesis have led to some of the wildest, I had almost said the drollest, writing upon the subject.

(*c*) In this category (but with many apologies) may we not venture to place the supposed *Muth und Stolz* ("pride and pluck" as Max Müller translates it) of the ancient Germans, to which Grimm is inclined to attribute the supposed *verschiebung* [3]? In such a conjecture we may recognize, with Baudry, an amiable and simple patriotism; but the great Teutone could hardly have been unaware that his explanation suggests too much; for, as Curtius remarks, with some humour, if we admire the courage of our ancestors in "raising" some

[1] *E.g.*:—" . . . What, for example, was a *p* in the original form of a word, or at least in the oldest form known, is found at a later stage transformed into *f*, which next passes into *b*; and this again tends to become *p*, and *go through the cycle anew*."—Chambers's Encyclopædia.

[2] See Appendix A.

[3] "Liegt nicht ein gewisser muth und stolz darin, media in tenuis, tenuis in aspirata zu verstärken?"—Gesch., p. 437.

of the mutes, we should equally bewail their pusillanimity in "lowering" the others[1].

(*d*) Closely akin to this conjecture is Grassmann's visionary Heroic Age, which he would elevate far above the reach of the great phonetic laws to which ordinary men are subject. What Grimm, however, modestly suggests in the form of a question, Grassmann decides sans *if* or *but*[2]; although he is equally forgetful to explain how his potent heroes could at once conquer their own innate physical tendencies (as in the change from **S** to **H**) and be conquered by them (as in the other and fourfold more extensive changes,—**H** to **A** and **A** to **S**). All, therefore, that the hypothesis gains is a new element of bewilderment.

(*e*) Any one who thinks it worth while may easily test the value of such fancies by selecting the most heroic age of the proudest people known to history (for there have surely been as brave men since as there were before Agamemnon) and investigating the amount and kind of corresponding variation in the phonetic character of the language of that people. Neither Grimm nor Grassmann has ventured to apply this simple test; and whoever does will find that no variation contrary to the ordinary natural rule is anywhere, during any such period, discoverable. The great deeds and series of deeds that we connect with exceptionally heroic ages are generally achieved in comparative silence; and where men *have* used language as an auxiliary, their mind and soul have stamped themselves, not upon their *alphabet*, but upon their *style*. Now we have no other safe guide to what might or should have been than what *has* been,—to the unknown than the

[1] "Wenn wir den muth unserer vorfahren in der erhebung von *d* in *t*, *t* in *th* verehrten, so müssten wir ihren kleinmuth in der senkung von *dh* in *d* bedauern."—Kuhn's Z-S., ii. 330.

[2] "Es gilt diese regel [our Principle of Least Effort] doch nur für solche perioden der entwickelung, in welchen die ursprüngliche schöpferische kraft erschlaffte, und einer allmählichen entartung und verweichlichung des volkslebens und damit auch der sprache platz machte,.... nimmermehr aber für eine zeit energischer kraftentwickelung, nicht für die heldenzeit eines volkes," &c., &c.—Kuhn's Z-S., xii. 100.

known. A belief in the uniformity of nature is as essential in Comparative Philology as in any of the natural sciences. To refer phenomena to a totally different order of things, supposed to have prevailed at an epoch which lies far beyond the reach of investigation, is not to explain difficulties, but only to slur them over; it is to trust for the acceptance of a doctrine to the possible absence of evidence to the contrary. Such conjectures, therefore, as those just quoted merit only so much attention as respect for their authors may prompt.

9.—(*a*) Other adherents of the Chronological Hypothesis have naturally been dissatisfied with these attempts to explain the discrepancy under consideration, and have looked about for some external agency which may account for it. One favourite resource of these is an imaginary action upon the primitive German tongue of some foreign race with which the German tribes may have come into contact. Thus M. Baudry[1], in combating a different opinion, remarks:—"Nous aimerions supposer que la confusion n'existait pas au commencement, mais qu'elle s'est produite à un instant donné, par suite d'une circonstance perturbatrice, *telle que le contact d'une race étrangère.*"

(*b*) Now, that the Germans, as well as the other divisions of the Indo-Europeans, may have come into contact with alien races, is highly probable. But when we consider the thoroughness of the phonetic changes to be accounted for, this explanation must be stretched to imply very much more than mere contact. Even extensive absorption, as the result of conquest, would, so far as our observation reaches, not be too much. Against this, however, the arguments seem to me overwhelming; but as it is hardly worth while to occupy much space in discussing the suggestion in its present vague and general form, I will merely point, by the way, to the singular homogeneity and strongly-marked individuality of the German dialects, High and Low alike. Except the English, which has been disintegrated during

[1] G. C., p. 146.

comparatively recent times, these all in every department—Grammar, Syntax, Vocabulary, Idiom, Phonology—convey the impression of uniform development under the guidance of a well-marked type of mind accustomed, in the expression of its thoughts, to rely above all things upon its own native resources. And even the English—which has been subjected to a succession of "contacts," and, indeed, to convulsions of a violence and duration that the advocates of "contact" would scarcely postulate in the case to which they apply it—shows how, notwithstanding all modifications of grammar, loss of inflexion, and disintegration and renovation of vocabulary, the *Mute-system* of a language maintains its place. Among the Vowels, however, (to judge from the history of the English vowel-system,) we should expect to find extensive traces of such contact. On the contrary, we find the primitive vowel-system maintained by the Germans with singular purity [1]. Among the conditions, therefore, to be satisfied by the alien race, it appears that their vowel-system must have been identical with that which the primitive Germans brought into Europe, while their mute-system must have differed in such a remarkable way as, by "contact" with the German system, not to corrupt it, but to transmute it, and not merely to transmute it, but to transmute it symmetrically with respect to the primitive or classical system with which it was, by the hypothesis, previously identical.

(*c*) But this is not all: upon one improbability is to be superimposed another still greater; for so far we have only taken the first (L.G.) supposed substitution into account; the second (H.G.) yet remains to be explained. And if "contact" is to account for the former, it should also account for the latter; or else the suggestion, being only applied to one

[1] See the Gothic system in Schleicher's Comparative Vowel-tabulation, Compend., pp. 156, 157; to which may be added the following from "Die D. Spr." (p. 91):—"Die hohe lautliche und formliche schönheit, die das Deutsche auszeichnet, und die, was das wunderbar lebendig erhaltene, ja weiter als in der Ursprache entwickelte vocalsystem betrifft, von keiner andern indogermanischen sprache erreicht wird, hat das Gotische am treuesten und reinsten erhalten, obwohl kein deutscher sprachzweig dieser vorzüge völlig enträth."

phonetic movement, is worthless. Now in the second case we must suppose contact with another alien race different from the first; for as the German mutes had already, by hypothesis, been adjusted to those of the first race, it would be absurd to suppose that that adjustment would be completely perturbed by a continued or renewed contact with the same race. Nevertheless we have here again to explain the continued existence of a vowel-system of remarkable purity (for the Oldest High German in this point differs very little from the Gothic) side by side with a transmutation of consonants, not merely symmetrical with the L.G. system (from which the H.G. system is supposed to be derived), but symmetrical therewith in precisely the same order and proportion as the L.G. system is with respect to the Cl. system. The improbability of an accident of this kind happening, as the result of contact, on the top of a previous similar accident really amounts to an impossibility.

10.—(*a*) But in order that this "contact" hypothesis may be subjected to a fitting scrutiny, it ought to specify the language or languages to which the several substitutions are to be attributed: we might then inquire whether the German dialects exhibit any of those changes which our observation and experience of linguistic commixture lead us to expect. Accordingly, there are not wanting philologists who have ventured to be thus specific. Dr. E. Förstemann, for example[1], attributes the substitution of **S** for **A** to Keltic, and that of **H** for **S** to Finnish, influence. The Germans, in their migration westward, overran and subdued extensive territories occupied by the Kelts, whose phonetic system was destitute of Aspirates. This contact, it is said, led to the deaspiration of all their primitive Aspirates on the part of the Germans. In spreading northwards the Germans similarly overran and subdued lands occupied by the Finns, whose Mutes then consisted of Tenues

[1] Gesch. des D. Sprachstammes. See also Prof. March, Compar. A-S. Gram., 1st ed., p. 29, note (*a*). The latter, however, appears inclined to attribute to the Kelts the two substitutions which are *not* attributed to them by Förstemann.

only. This contact had the effect of raising to Tenues all the primitive German Mediæ.

(*b*) Here unfortunately terminates the list of specified contacts; and it is consequently either too short or too long. For if contact was or could have been the cause of the supposed substitutions, then, to be consistent, we want a good deal more of it. The two substitutions attributed to it do not cover half the L.G. Mute-system alone (§14 *infrà*): to account for the remaining substitution (**A** for **H**) a far more forcible and effective contact remains to be discovered; while for the series of H.G. substitutions a fresh series of successive contacts is required. On the other hand, if the larger part of the L.G. and the bulk of the H.G. consonants were transmuted by some other agency than contact, where is the use of imagining the two contacts just specified in order to account for so comparatively limited a part of the whole phonetic movement?

(*c*) But let us accept the explanation so far as it goes: then, considering the very thorough transmutation effected by each of the two suggested contacts in that section of the mutes which it selected for its operation, we have a right to expect that some of the other and more usual effects of linguistic mixture should likewise be distinctly pointed out. These effects are well known; indeed, they may almost be formulated, in outline, thus:—If two different races, r and r', whose languages are l and l', undergo fusion, the resulting language L lies intermediate to l and l', and, *ceteris paribus*, approaches more nearly to l or l' as r, in point of number and importance, is greater or less than r'. Of course, if r and r' be nearly related, l and l' approximate in corresponding ratio; and L will differ less from either l or l' than each of these differs from the other. These results hold good in every department of language; but to apply and exemplify them fully would require a volume to itself. My time and space allow me only to indicate, as briefly as possible, two or three leading departments in which they ought to appear.

(*d*) And first as to *Phonology*. Whatever sounds in l and l' are exactly alike, can, of course, be readily pronounced by

both r and r'. But where the sounds of l (say) differ from those of l', the attempts of r' to acquire them will, in some cases at least (*i. e.* when they offer any difficulty), result in approximations only,—that is to say, in modified or merely imitative sounds which r' can produce with greater ease than the genuine ones. And as the natural tendency is towards debilitation, r will readily stoop to the infirmities of r', until at last some intermediate sounds are universally accepted by both races. The same is true of r' and r in reverse order; and thus phonetic contact means phonetic compromise; and one of the most striking results of linguistic fusion is an accelerated phonetic debilitation. If, then, the most ancient German underwent a twofold admixture, the results of which are so striking among certain sections of the mutes, we should naturally expect to find likewise some signs of a general corruption among the whole body of sounds. In point of fact we find nothing of the sort. Even the changes attributed by the hypothesis to the specified contacts are not corruptions at all, but systematic transmutations, one of which (that from **S** to **H**) is exactly the reverse of debilitation.

(*e*) Next, as to *Grammatical Structure*,—or, at least, Declension and Conjugation[1]. Here the prevalent doctrine is that in the Grammar of a language there can be no mixture[2]. This may be true; but I am not sure that the proofs are perfectly satisfactory. In all the favourite examples (modern English to wit) it will be found that there has been a considerable preponderance, either of numbers, or of political and social importance, or both, on the side of one or other of the commingled races. We are not yet in possession of any induction from a sufficient number of cases in which r and r' were pretty nearly equal in those respects. From what is to be seen where they have been unequal, I am inclined to think that if two dissimilar languages, in what may be called their natural state, could be fused upon precisely equal terms, the

[1] I take the case of Inflexional Languages only; our observation and experience are but just beginning to travel beyond these.

[2] See an excellent chapter on the subject in Mr. Sayce's "Principles of Philology."

inflexions of both would at last entirely disappear[1], and totally new methods (like the French future tense and prepositional noun-cases) would be invented for expressing *all* grammatical relationships. In this case, it would still be true that there would be no mixture of Grammar; but the possibility of mixture would be prevented by mutual destruction. To this state of things the grammar of some modern mixed languages (English itself, for example) offers no very distant resemblance. Nor is the reason past finding out. For the inflexional appendages of *l* mean nothing to *r'*, those of *l'* nothing to *r*; it is as much as the people on each side can do to catch up the central word with its general meaning. Soon, therefore, those appendages begin to be peeled away, and their places to be supplied by separate particles and notional words, which are few in number, easily acquired, and useful for many additional purposes. In the matter of Inflexion, accordingly, still more than in Phonetics, contact means accelerated corruption. Consequently, among the Old Germans, on the hypothesis under review, both the Kelts and the Finns should have initiated a period of grammatical decay, and the effect of the two together ought to have been very striking. What is more, the O.H.G., which for consistency's sake (See subsection (*b*) above) should exhibit the effects of a sixfold contact, ought to have been completely stripped of its inflexions. But the actual state of the facts is just the reverse. In the Oldest L.G. no corrupting agency, beyond that of uniform and characteristic native debilitation, is to be detected at work; while, more remarkably still, the Oldest H.G. is particularly rich in inflexions, and its descendent dialects are at this day inflexional to a higher degree than most of their contemporary sister dialects.

(*f*) But the state of the Old-German Grammar and Pho-

[1] Or, if there were any remains, I see no necessary reason why *l* and *l'* should not both contribute thereto. We have an indication of the possibility of such a result in English; where, although the structure is in the main Teutonic (as it ought to be by (*c*) above), yet the universal prevalence of the noun-plural in *s* is due entirely to French influence and example.—See Earle's "Philol. of the Engl. Tongue," 2nd ed., pp. 350-352.

nology proves only that there can have been no *extensive* commixture of races. For slighter degrees of contact we require another test; and this is furnished by the *Vocabulary*. There is scarcely any degree, however slight, I will not say of actual contact, but even of communication, with an alien people, which is not sufficient for the introduction of some of their words. Travel, Commerce, and Education are here as effective as Conquest. English, for example, abounds in words collected from almost every country under heaven; and such words might be arranged in a numerical scale which would roughly indicate the extent of communication with the several peoples from whom they have been borrowed. This test, applied to the suggestions noticed in subsection (*a*), is decisive against them. Förstemann, with every disposition to succeed, fails to produce from the Oldest German a single word that can *certainly* be traced to the Finnish[1]; and although, as might be expected, he produces several which appear to have come from the Keltic[2], he is not sure but that even these may have spread at a later period through the leading German dialects from such of the German tribes as are known to have really come into contact with the Kelts. To talk, under these circumstances, of Keltic and Finnic influence as so unerringly selecting and transmuting, as if by systematic and concerted action, first the Aspirates and then the Mediæ of the Old German Mute-system, is mere trifling.

(*g*) The truth is that the differences between Σ_x, Σ_y, and Σ_z are of a totally different character from those known to be attributable to the fusion of one race and language with another, and require a totally different treatment and explanation. Grimm's Law formulates neither fusion nor confusion. What we must keep our eye on is the harmonious, equipollent, symmetrical, and functional relationship of all those systems *inter se*; and that relationship is adjusted upon a plan of which the interpretation is not to be read in the chapter of accidents. But before we quit this part of our subject, it is pertinent to remark that the experiment of subjecting one and the same language to the influence of alien

[1] Gesch., p. 610. [2] Id., p. 607.

races, both unrelated one to another and of all degrees of relationship, has been tried for us on a large scale, and, as it were, under our very eyes. I refer, of course, to the spread of the Latin tongue among the peoples of Southern and Western Europe, whence has resulted the formation of the Romance languages and their multifarious dialects. Here, if anywhere, we might fairly expect to find, on the "contact" hypothesis, numerous and very striking examples of apparently correlated phonetic action similar to that formulated in Grimm's Law; but here we do *not* find them; and it will hereafter appear that the condition of independent development under which those languages grew up furnishes the very reason *why* we do not find them.

11.—In approaching the consideration of the most generally received form of the Chronological Hypothesis we must first notice the discussion as to where, on that hypothesis, Substitution originated,—that is to say, as to which of the three possible changes (**H** to **A**, **A** to **S**, or **S** to **H**,) came first in order of time. And here it is perplexing to find, at the outset, that each of the three has, by some leading philologist or other, been selected as the initial one. Thus Grimm[1] starts with the change from **S** to **H**; and so, with *muth*, if not with *stolz*, puts one of the least likely changes in the forefront. Bopp[2], Schleicher[3], and others[4] take the supposed substitutions in the order **H** to **A**, **A** to **S**, and **S** to **H**. The third and only remaining case possible, which begins with the change from **A** to **S**, has, in its turn, been propounded by no less eminent a scholar than G. Curtius[5]. Regarded from the stand-point furnished by the Principle of Least Effort, the view of Bopp is, as we shall presently see, the least objectionable. As, however, the third view (that of Curtius) appears to harmonize better than the others with some of the

[1] Gesch., p. 344 *et alibi.*
[2] V. G., § 87.
[3] Die D. Spr., p. 97 *et seqq.*
[4] *E. g.*, Whitney (Jolly), p. 154; and Grassmann in Kuhn's Z-S., xii. 96.
[5] "Die Aspiraten der I-G. Sprachen:" Kuhn's Z-S., ii. 322, &c.

phenomena to be explained, as it has been expanded into a complete theory of Grimm's Law, and as it has been very widely accepted, I shall take the liberty of selecting it for a somewhat minute examination.

12.—As an indispensable preliminary, we must glance at the same learned author's theory of Aspiration, under which the aforesaid initial substitution is ranged. By this theory each of the Aspirates is assumed to have been originally a compound and soft sound (=media+breath), capable, indeed, of combination into a simple sound, but also, under other circumstances, separable into its components, each of which might either absorb or fall away from the other, pretty much as if they were still independent sounds in mere juxtaposition. That is to say:—(i) the breath might vanish, leaving the media alone; (ii) the media might vanish, leaving the breath alone; and (iii) media and breath, by fusion, might produce a spirant. To these Curtius adds the startling postulate (iv) that the breath might also *raise* the accompanying media into a tenuis, and then, by continued association with it, give rise to the hard Aspirate. Result (iv) certainly has the aspect of an excrescence; it has already been assailed by Max Müller[1]; and I shall endeavour hereafter to show that it is as needless as it is unlikely[2]. But neither this nor the results marked (ii) and (iii) concern us just now. The characteristic of the theory clearly lies in the composite aspect under which it represents the primitive Aspirates, and the loose cohesion it attributes to their components. This is essential to its connexion with the *Lautverschiebung*; for it is by supposing the breath to fall away from the media, as in (i), that we are to obtain the substitution of **S** for **A**, which Curtius assumes to have been the initial one. This starting-point is not without appearances in its favour; for where **A** occurred, by the hypothesis, in the

[1] Lectures, ii. 223, *note*.

[2] It is introduced to serve as a basis for an explanation of the awkward fact that all the Greek Aspirates were *hard* Aspirates; which quality must be reconciled, by any means, with the supposed *softness* of the old Aspirates. (See §§ 32, 33 *infrà*.)

Holethnic speech, **S** occurs in later languages on a very extensive scale, as, for example:—throughout the L.G. dialects, throughout the Li-Sl., generally in the Old Keltic and Old Persian; often in the Zend; and nearly always in Latin, and sometimes in Greek, in the case of internal consonants. Besides, Lottner, towards the end of his well-known article on the Exceptions to the First *Lautverschiebung*[1], has acutely pointed out that the change from **A** to **S**, as between Σ_x and Σ_y, is by far the most thorough and complete of the three changes—which seems[2] to warrant the conclusion that that change had set in soonest, and had therefore been in operation longest; and Grassmann ("Ueber die Aspiraten," &c.)[3] claims to have furnished fresh evidence in favour of the same view by establishing the prevalence of similar deaspirating movements both in Greek and Sanskrit.

13.—Now, in spite of the suspicious looseness of composition and the protean changes attributed by this theory to sounds which might rather be expected to exhibit somewhat of consonantal unity and rigidity, let it be provisionally granted that an initial substitution of **S** for **A** may thus be accounted for: it next becomes necessary to see what advantage accrues therefrom to the other substitutions. To explain these, Curtius supposes that when the first substitution was completed, or at least as it advanced towards completion, a Discriminating Impulse or Instinct (*Unterscheidungstrieb*), a sort of Differentiating Principle of language, came into play. Men were conscious that the *old* Mediæ were not the same as the *new* Mediæ which sprang from the primitive Aspiratæ, and so the old *g, d, b*, were gradually strengthened to *k, t, p*. But these new Tenues, in their turn, clashed with the old Tenues; and the latter therefore began to differentiate themselves from the former by severally assuming a thick breathing after them; and thus were produced the genuine Aspirates *kh, th, ph*; which afterwards became weakened to their related Spirants. Such

[1] Kuhn's Z-S., vol. xi.
[2] But perhaps only seems. See § 50 (*c*) *infrà*.
[3] Kuhn's Z-S., vol. xii. See Appendix F, *infrà*.

is the original statement of Curtius; and it agrees substantially with the views expressed in his Greek Etymology[1],—the main difference being that, instead of the Differentiating Impulse, he here invokes Reciprocal Compensation as the operative cause of the later substitutions.

14.—(*a*) Now, however ingenious a theory may be (and the ingenuity of the theory before us is beyond dispute[2]), its real value is unfortunately to be estimated, not so much by the difficulties it solves as by those it leaves unsolved. It may harmonize with ninety-nine facts, but it may be irreconcilable with the hundredth; in which case we have no option but to suspend our assent on account of its failure in the one point, rather than to accept it too readily for its success in the ninety-nine. In the theory of Curtius I think I see more than one essential point which is scarcely of a kind to command acceptance. I do not say much at present on the apparent absence both of motive and of purpose in the assumed initial substitution, and indeed in all the rest—although it is, on the face of it, more than extraordinary that a dialect to which, by the hypothesis, the Cl. mutes were originally *native* should spontaneously modify the whole series with scarcely any other ultimate result than a mere change in their distribution. There was, it seems, no addition to their number; and as the Cl. system still sufficed for the Cl. tribes, we may assume that the phonetic requirements of the Germans likewise must already have been fully supplied. And similarly, as to the second *Verschiebung*, the L.G. mute-system, in its turn, is supposed to have been at one period *native* to the H.G. tribes; and as that system continued to suffice for the L.G. tribes, and as the

[1] Third edition, pp. 425-6.

[2] "Das Lautverschiebungsgesetz der germanischen Sprachen ist eine viel zu wichtige sprachliche Erscheinung, als dass sie nicht seit ihrer Entdeckung schon eine Menge Versuche sie zu erklären hätte hervorrufen müssen; unter diesen dürfte aber die von Curtius in Kuhn's Z-S. ii. 322 ff. aufgestellte als *eine wohl gelungene* zu bezeichnen sein."—Whitney (Jolly), p. 155, *note*. Curtius's views have been adopted by Lottner, Grassmann, Förstemann (throughout his important "Gesch. des D. Sprachstammes"), and a host of other writers.

second *Verschiebung*, like the first, merely led to a revolution, and not to an extension or any perceptible improvement, of the L.G. system, we may assume that the latter system was amply sufficient for the requirements of the H.G. tribes also. No other example, I suppose, in the whole world can be produced, in which a people has thus—I will not say twice, but even once—spontaneously revolutionized by far the firmest section of its articulate sounds.

(*b*) To assert that all this supposed change really *was* as wanton and even mischievous as it appears, would perhaps at this point be inconsistent with the caution befitting our inquiry; for both cause and purpose may lie beyond our ken. But what we may safely say is, that, on the hypothesis under review, the first substitution *appears* causeless, purposeless, and (if it really necessitated the others) mischievous. It may deserve passing mention, however, that some adherents of this theory have discovered for us a cause so widely dissonant from the facts of the case, that one finds it hard to believe that sufficient attention has been paid to those facts. We are told that the Aspirates were "objectionable"[1]. No doubt they were so in some quarters; and this very fact will be hereafter applied to elucidate a totally different set of phenomena. But to the assertion that the Aspirates were objectionable to the ancestors of the Low Germans, I altogether demur. From the evidence at our disposal I should infer precisely the reverse. But to make good my point I will fall back on independent authority.

(*c*) Nearly a quarter of a century ago Dr. E. Förstemann executed a series of calculations to determine the relative proportions in which the several articulate sounds, vowel and consonantal, enter into some of the leading I-E. languages[2]. He seems to have had no immediate object in view beyond establishing facts; and as he could not possibly foresee to what use those facts might be turned, his testimony must be

[1] "The first variation [*i. e.* the supposed L.G. *Lautverschiebung*] rose, as all (?) agree, from the objectionable Aspirates."—Peile: "Grk & Lat. Etym.", 2nd edit., p. 157.

[2] Kuhn's Z-S., i.; and more particularly ii. 38.

held to be completely free from bias. Taking first the principal Classical languages, he obtains the following results as to the *Mutes*[1]:—

	Skt.	Grk.	Lat.
Tenues	52·6 per cent.	71·4 per cent.	72·0 per cent.
Mediæ	29·0 „ „	14·3 „ „	25·5 „ „
Aspiratæ ..	18·4 „ „	14·3 „ „	2·5 „ „

Striking the average, we may attribute to the *Classical* Mute-system

65·6 per cent. of *Tenues*,
22·7 „ „ *Mediæ*,
but only 11·7 „ „ *Aspiratæ*.

Compare with this the *Gothic* Mute-system, which, as he next shows, comprises

14·3 per cent. of *Tenues*,
22·9 „ „ *Mediæ*,
and no less than 62·8 „ „ *Aspiratæ*[2];

[1] I have, for convenience, reduced his proportions to percentages; and I am responsible for the averages.

[2] It does not appear from Förstemann's articles on what lines of inquiry he advanced—on how many words, for example, he based his calculations—whether he took the words from a dead vocabulary, or from living compositions—and whether, in the case of Gothic, he includes not only actual substitutions, but those which *would* have been made but for some overruling circumstance, such as "protection" by *s*. I raise these doubts because, before I had unearthed those articles, I had commenced a similar series of calculations in relation to the mutes alone, but with results somewhat less strikingly in favour of my argument in the text, although still striking enough. Thus, for the Cl. system, taking the primary words (above 900) in the primitive I-E. vocabulary forming the first division of Fick's W-B., I found

Tenues 55·5 per cent., *Mediæ* 24·0 per cent., *Aspiratæ* 20·5 per cent.; and results almost identical were obtained by digesting the *initials* of *all* the words in the same vocabulary; while in Homer, as representing the older Greek, out of above 700 mutes examined, there were

Tenues 59·7 per cent., *Mediæ* 22·3 per cent., *Aspiratæ* 18 per cent.

In elucidation of the L.G. system, I took the *initials* (as these are in general most regularly *verschoben*) of all the words (about 1100) in Fick's primitive German vocabulary; among which I found

Tenues 17 per cent., *Mediæ* 35 per cent., *Aspiratæ* 48 per cent.

With this result may be compared that of an examination of the mutes

that is to say, among the people to whom the Aspirates are asserted to have been "objectionable," Aspiration prevailed five or six times as extensively as it did among those to whom they were not so.

(*d*) Never, surely, was there suggested a motive for any given line of action so completely inadequate to explain the same, or so flatly contradicted by facts. It would have been a far more cogent argument to attribute the change from Σ_x to Σ_y to the very marked, the really extraordinary, natural aptitude and affection for Aspiration on the part of the L.G. peoples. This would bring us over to the second view (that of Bopp and others) stated in § 11, which thus accounts, at a stroke, for three fifths of all the supposed consonantal transmutations. But some mystery would still overhang such an explanation; for one would have thought that so strong a love for Aspiration would certainly have led the Germans to maintain the Aspirates they already had, and to increase their number, if impelled to do so, by modifying other mutes.

(*e*) It cannot be argued that this large percentage of the newer mutes thus finally obtained were not really Aspirates but only Spirants; for, not to mention the distinct assertion of Curtius to the contrary [1], such an argument would deprive the theory under review of much of its value, and would in fact render a second necessary in order to account for the evolution of Σ_z from Σ_y. To maintain one and the same hypothesis consistently throughout both of the supposed evolutions, it should be maintained that the L.G. Aspirates (not Spirants) were similarly objectionable to the H.G. tribes,

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(nearly 1800) in the episode of Ohthere in King Alfred's Orosius, where there appear

Tenues 22·5 per cent., *Mediæ* 29·2 per cent., *Aspiratæ* 48·3 per cent.

I had intended to extend my examination to the mutes, both in vocabulary and in composition, of all the leading Cl. and L.G. languages; but it is much easier to borrow statistics than to compile them.

[1] "Das alte *k, t, p* konnte es sich nicht gefallen lassen mit dem neuen auf einer stufe zu stehen. Ihm stürzte ein dicker hauch nach, *der anfangs sicherlich wahre aspiraten (kh, th, ph) erzeugte*; von denen *kh* und *ph* zu *h* und *f* sich verflüchtigten."—Kuhn's Z-S., ii. 331.

and that the desire to avoid them led to the initial movement towards the second *Verschiebung*[1]. Besides, if the new L.G. sounds had been Spirants, their supposed components would have been fused beyond the power of the H.G. tribes to separate the breath from the other element (which, by the way, in this case, is *not* the Media[2]).

15.—(*a*) Returning from this digression, I venture to suggest that the *nature* of the supposed change (**A** to **S**), which is the corner-stone of the whole hypothesis, is, in itself, not altogether beyond challenge. As was pointed out by Schleicher many years ago[3], the natural tendency of the Aspirates within historic times and in individual languages has been to become, not unaspirated mutes, but Spirants; and this tendency he went so far as to call "a law arising from physiological causes." Now, although there is room to doubt whether the genuine Aspirates ever passed into Mediæ by natural and unconstrained debilitation, I would not point-blank deny the possibility of such a transmutation, at least under the influence of neighbouring sounds. But what cannot fail, in Curtius's theory, to strike us at a glance, is the incongruity that, up to a certain limit of time, the tendency of the whole body of Aspirates ran in one direction (*i. e.* towards the Media), while from that limit it has run in a different one (*i. e.* towards the Spirant). This would arouse suspicion if it were chargeable upon two different races of people; but when it is charged, and that twice over, upon one and the same race, it approaches the incredible.

(*b*) For observe what we are really asked to believe, namely: —(α) that the original Cl. Aspirates among *all* the primitive Germans became Mediæ; (β) that the second crop of Aspirates (supposed to result from the third substitution, **A** for **H**, in Σ_y) generally became, among the Low Germans, Spirants; (γ) that, among the High Germans, of this same crop part

[1] See § 16 (*c*) *infrà*.

[2] See the following § (*c*).

[3] "Formenlehre der K-Sl. Spr."—quoted, in fact, by Curtius in the article so often referred to.

also became Spirants[1], but the greater number passed into Mediæ; and (δ) that of the third crop of Aspirates (supposed to result from the third substitution, **A** for **H**, in Σ_z) some have in H.G. become Spirants, and the others are arrested at a stage which is much nearer to the Spirants than to the Mediæ. It is more than odd that one and the same sound, among one and the same race of people, and, so far as we know, under one and the same set of circumstances, should have exhibited these incongruous modes of development. Of the passage of Aspirates into Spirants we find ample historical evidence—in Skt, in Grk, in Latin, in L.G., in H.G.; but in favour of their transmutation into Mediæ we have only a fact which may mean something quite different—namely, that while Aspiratæ appear in certain words of some languages, Mediæ appear in the corresponding words of related languages.

(c) In the way of the contested transmutation (**A** to **S**) indeed, as between Σ_y and Σ_z, Curtius's own hypothesis introduces a new and unexplained difficulty. For by that hypothesis the L.G. Aspirates, forming 60 per cent. of all the L.G. Mutes, were *hard* Aspirates (§ 14 (*e*), *note*); and if the Cl. Aspirates (supposed to be *soft*) were severally equal to *media* + *breath*, and by loss of breath gave rise to the L.G. *mediæ*; the L.G. *hard* Aspirates ought severally to be = *tenuis* + *breath*, and, by the loss of the breath, ought to give rise to *tenues* in H.G.; which, when deaspirated, as supposed, they do not. There is consequently wanting somewhere an intermediate stage of debilitation. Or are we to suppose that the breath, which in Greek *raised* the media to the tenuis (§ 12 *antè*) and then combined with it into the *hard* aspirate, here *lowered* the tenuis to the media, and combined with it into the *soft* aspirate, before the latter passed into the H.G. media?

16.—(a) So much with reference to the initial substitution;

[1] This qualification is inserted to meet the view which represents as *unverschoben* those H.G. *f*'s and *h*'s which agree with the same Spirants in L.G.

which, on the hypothesis under review, is apparently causeless and purposeless; which attributes to the primitive Aspirates a bisonant and oscillating character, open, to say the least, to considerable doubt; which, in its execution, involves more than one striking inconsistency; and which, as we next have to notice, is represented as directly or indirectly necessitating other changes which, with it, amount to nothing less than a phonetic revolution. For the supposed debilitation of the Cl. Aspirates had, it is said, the immediate effect of inciting to the strengthening (in the second substitution, **H** for **S**) of twice as many Mediæ into Tenues, and ultimately involved the change (in the third substitution, **A** for **H**) of four times as many Tenues into Aspiratæ.

(*b*) As no one has asserted more distinctly than Curtius[1] that "Weakening" is the ruling principle of phonetic change, and as he even rejects[2] Max Müller's counteracting principle of "Dialectic Regeneration" on the ground of its inconsistency therewith, we are naturally curious to learn the nature of those more potent forces to which he would attribute such changes as the above-mentioned second substitution (**H** for **S**). Their names, as we have seen, are Discrimination and Compensation,—names which may really be intended to denote different aspects of the same process[3]—the former perhaps directing attention rather to some unperceived mental instinct originating the second and third substitutions, the latter to the relationship which those substitutions assumed in course of execution.

[1] Grk Etym., p. 412. [2] Ib., *note*.

[3] Compare the following passage from the "Grk Etym." (pp. 425-6) with the summary of Curtius's older explanation in § 13 *antè*:—"Der Uebergang von *g, d, b* in *k, t, p* in den Germanischen Sprachen erklärt sich aus jenem Zusammenhange, der zwischen sämmtlichen Lauten einer Sprache in der Art stattfindet, dass sich diese wechselseitig compensiren. Die einmal eingetretene Verwandlung eines *dh* in *d* trieb auch das ursprüngliche *d* aus seiner Stellung, so dass das alte *d* zu *t* ward, und endlich das neue *t* wieder das schon längst vorhandene alt überlieferte zu *th* verschob." The repeated *statement* that one sound "*verschob*" another is, of course, no *explanation* of the "*Verschiebung*." Nor is it quite self-evident how the change of *t* to *th* can, properly speaking, "compensate" for that of *d* to *t*, or the latter change for that of *dh* to *d*. But see §§ 18-22, *infrà*.

(*c*) Of what Curtius means by the former (to speak of this first) we can acquire some conception from its supposed mode of operation. This, as it affects the evolution of Σ_y from Σ_x, is represented as follows:—When by phonetic change in a given language a whole series of consonants of a certain degree became assimilated to another series of another degree, the latter series was made to change its degree throughout; and when this series consequently became assimilated to a third series, this third series was also made to change its degree throughout. The singular result in the case before us was, that the language thus acquired, in fivefold measure, the very sounds which, for some unknown reason, it endeavoured at the outset to get rid of. To put this process in another light—a language which, under the sway of its debilitating tendencies, was powerless to maintain a single distinction (viz. that between **A** and **S**) already existing on a small scale, is nevertheless supposed to have elaborated two other fresh distinctions on a much larger scale, and partly in direct opposition to such tendencies. To account for the evolution of Σ_z we ought, of course, to suppose a repetition, in different proportions, of the same set of changes. But here we meet with a difficulty. For, as is well known, the existing H.G. labial and guttural families, in addition to their own Spirants which properly answer to the L.G. Tenues, also exhibit Spirants (*f* and *h*) in correspondence with the L.G. Spirants *f* and *h*, and where, on Curtius's hypothesis, they should exhibit the Mediæ *b* and *g*[1]. Nevertheless, although in these cases the **A** of Σ_y is said not to have proceeded to **S** in Σ_z, we still find the **S** of Σ_y apparently raised to **H** in Σ_z (*i. e.* *b* and *g* to *p* and *k*). By what power, then, was this transmutation effected, or at least necessitated?[2]

[1] But see §§ 52–54 *infrà*.

[2] It was Dr. (now Prof.) W. Scherer, in his acute work "Zur Gesch. der D. Spr.," who, if I remember right, first raised this objection to Curtius's hypothesis. Scherer's own view is that the successive substitutions were totally independent of one another; and he gives an elaborate plan of the way in which he supposes the evolution of one sound from another to have taken place. I am not aware that this view has gained any converts, except perhaps Dr. Schweizer-Sidler (See Kuhn's Z-S.,

(*d*) But if all such difficulties were obviated, the whole process would still savour little of the economy of nature. If the initial change actually had taken place as is supposed, no great harm would have been done; and we are at a loss to see either why any remedial measures were necessary, or, if necessary, why they should involve other changes sevenfold more extensive and partly in direct contravention of nature's ordinary modes of procedure. One would think it much simpler, and almost infinitely easier, for a discriminating principle to have exerted itself in causing the immense majority of Mediæ and Tenues to *resist* the supposed tendency of the Aspiratæ, and thus to keep them in their original places. Such strange movements of language, however, are appropriately relegated to a period beyond observation; for they seem quite out of harmony with any of the movements which history has yet enabled us to watch. These, with few and generally explicable exceptions, all tend in one direction. When certain results have been achieved, men appear quite indifferent to the nature of such results, and incapable of altering them, however inconvenient they may be[1].

17.—(*a*) Now the chief if not the only practical inconveni-

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xviii., p. 289). It is partly for this reason that I do not select it for special examination, and partly because, as my own central idea is precisely the reverse of it, all the arguments and evidence I may hereafter adduce will cut directly or indirectly against it.

[1] One further difficulty I have not stated in the text, because Curtius himself makes no allusion to the point that gives rise to it. I should judge, from his description of the supposed successive substitutions, that for their complete execution a very long period of time would be necessary. Thus the original Aspirates would have died out long before the Low Germans proceeded to evolve the second set of Aspirates out of their older Tenues. By what means, then, was the memory of the Aspirates, or at least the impulse to reproduce them, kept alive from generation to generation? Förstemann (Gesch., i. 14) manifestly feels this difficulty in reference to a long period: for a short period he holds the necessary *Sprachgefühl* "nicht für unmöglich". All I would here remark is, that if a given sound has completely died out of a language, it can only be recovered when it forms the terminus to which, by natural debilitation, some still existing stronger sound tends.

ence arising from the assimilation of two series of consonants, such as the Aspiratæ and Mediæ of Σ_x, would perhaps be the formation of a small number of homonyms. In this respect the primitive German language, on the hypothesis in debate, would only have shared the fate of most, if not all, other languages, and to a far more limited extent than some of them. A monosyllabic language, indeed, like the Chinese, is but, as it were, a cluster of homonyms. A word of a single syllable may have to serve for a score of meanings; and its application in any given case has to be determined by such indirect means as emphasis and intonation[1]. The reduction of originally different words to the same sound displays itself, though far less extensively, in modern European languages, especially those which, like the French, have been reconstructed out of the *débris* and *detritus* of an older language. But little or no mischief is done; for, in the first place, the meanings of the several primitives are in general so widely different, that the homonymous derivatives remain to all time clearly distinguished in use; and, in the next place, since modern languages appeal quite as much to the eye as to the ear, means have been offered, by the fluctuating value of letters, for perpetuating symbolic distinctions after phonetic differences have disappeared[2].

(*b*) Nowhere, however, do we find (where we can observe) that Nature has been so busily fastidious as to remedy any such apparent confusion[3]: why, then, are we to assume that

[1] See Mr. Sayce's "Principles of Philology," p. 103.

[2] *E. g.*, in our *pare, pear*, and *pair*; or the French *ver, vert, verre*, and *vers*; etc. Here, if anywhere, in connexion with the purely artificial process of writing, a sort of rudimentary differentiation may be observed in operation—not, however, actively, by special adaptation of varieties of spelling to varieties of meaning, but (if it is not a contradiction to say so) passively, by a refusal to allow the spelling to proceed to complete assimilation, even after the fusion of the sounds.

[3] "Quand une langue dérivée perd la faculté de prononcer certains sons qui faisaient partie de celle d'où elle sort, il en résulte tout simplement des homonymes, et *la langue nouvelle n'y échappe pas.* Ainsi, le Zend convertissant les aspirées du Sanscrit en pures, les deux racines *dhā-* (poser) et *dā-* (donner), τίθημι et δίδωμι, s'y confondent dans un même verbe *da-* qui signifie à la fois 'donner' et 'faire.' Il en est de même de *-dere* dans

she interfered where our observation cannot reach? If, indeed, differentiation had set in among complete homonyms only, the limitation of her interference and the apparent reason for it might have passed for evidence that she really had for once relaxed her passionless indifference; but the interference is supposed to have gone on right and left, even between words which, after the first substitution, had but one letter in common [1].

(*c*) But further, the assimilation of the classical Aspirates to the Mediæ would only produce (§ 14) a series of Mediæ making altogether less than 40 per cent. of all the mutes; while the Tenues would still form upwards of 60 per cent. Among the words involving Tenues we should therefore conjecture that there must have already existed many more homonyms than among those involving Mediæ, new and old together. Now here was a fine opening for the application of some corrective process on the part of Nature; but none was applied: those numerous Tenues were all (or nearly all), by hypothesis, transmuted into Aspiratæ, and must consequently, in their new form, have perpetuated any disadvantage arising from the existence of homonyms. The action of the discriminating principle thus invoked appears therefore to have been somewhat *un*discriminating. Nor was it quite so impartial as one could wish. The early Germans, and they alone, were the chosen race among whom it is represented as manifesting its powers. The Slavonic tribes, who are supposed to have accompanied the Germans in the first substitution, seem, for no apparent reason, to have been left in impotence. And the

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les composés latins: il signifie tantôt 'donner' (*red-dere*) et tantôt 'mettre' (*ab-dere*, *con-dere*). Le chinois surtout est plein de ces homonymes produits par des ressemblances de mots usés, auxquels on peut comparer le français *tour*, de *turris* et de *tornus*; *somme*, de *somnus* et de *summa*, etc. Une fois que la dégradation les a réduits à cet état, le langage est bien forcé de les garder et de les éclaircir par des moyens indirects. *A moins d'une convention expresse, également impossible à imaginer et à mettre en pratique, jamais on n'aura recours à l'expédient de les distinguer par des prononciations nouvelles.*"—Baudry: G.C., p. 145.

[1] "Das junge *d* von Goth. *dêths* (root *dhâ-*) verschob das alte *d* von *dam-* zu *t* im Goth. *tamja*." *Cui bono?*

same is partially true of the other races, among whom the change from **A** to **S** was less extensive.

(*d*) There is undoubtedly a Differentiating Principle at work in language; but the one we are acquainted with acts in a precisely opposite way to the imaginary principle just discussed. Far from overruling or interfering with ordinary phonetic change, it is not only powerless against it, but actually dependent upon it—accompanying or following it, and taking advantage of its operations for the purpose of extending the powers of language. Its usual mode of procedure is this:—Two or more phonetic varieties of a word are produced by vowel- or consonant-change in the intermingled dialects of one and the same language: for a time they coexist; and if the idea expressed by the original word be simple and indivisible, then (unless the dialects are tending to separate, in which case each dialect takes with it its own single form), one of these forms ultimately prevails over the others, and these others gradually disappear; but if that idea be capable of variation or subdivision, then varieties or offshoots thereof are gradually assigned to the phonetic varieties of the one original word. On this subject there is a powerful *consensus* of opinion among the leading philologists, some of whom are hereinafter quoted[1]. And, if I mistake not, it is this well-known principle, and not the other strange and hypothetical one, which, as I shall presently try to show[2], may have contributed towards the evolution of the phenomena summarized in Grimm's Law.

18.—(*a*) We now turn to the other (if it is another) principle ("Reciprocal Compensation") which is supposed by Curtius[3] to produce the same effects as the imaginary Differentiating Principle examined in the foregoing section. As Curtius leaves the matter, indeed, it is by no means easy to see how the term Compensation is to cover the phonetic movement which is supposed to take place. Nevertheless, inasmuch as Compensation, strictly understood, really offers, as it seems to me, a first approximation to an explanation of

[1] Appendix C. [2] *Infrà*, §§ 27–30. [3] *Antè*, § 16, *note*.

Grimm's Law, I here propose to enter upon a brief investigation of one aspect of that process, partly for future reference, and partly with the view of further testing Curtius's representations.

(*b*) Compensation may conveniently be divided into two species—one between neighbouring or contiguous, and the other between distant and disconnected, sounds. The former species (which does not concern us here) has been carefully studied and duly enrolled among the most important of the elementary principles of Phonology. Of the latter I have not yet met with any attempt at systematic treatment. It consists either in dropping a sound where it should be heard and inserting it where it should not—or in modifying one sound, whenever it occurs, into another and closely related sound, and, in return, modifying the latter, whenever *it* occurs, into the former. These two varieties are really identical: their apparent difference is due to the nature of the sounds operated on. When these are already so feeble that no further debilitation is possible, except complete loss, then the first variety presents itself; but when they admit of further degrees of weakening, then we meet with the second variety.

(*c*) This process generally manifests itself in the popular and illiterate, as contrasted with the standard and literary, dialect of a language; for the former is unhampered by those conditions which contribute to the comparative fixation of the latter and which enable it to resist a too rapid debilitation. To formulate the process, let us put D for the standard, D' for the popular dialect, and s, s' for two related sounds affected by the movement in question: then, supposing that where D exhibits s, D' begins to exhibit s'; it may be found, contrariwise, that where D exhibits s', D′ will exhibit s. The resulting relationship may be tabulated thus:—

$$\left\{\begin{matrix} D \ . \ . \ . \ s \quad s' \\ \qquad\qquad \times \\ D' \ . \ . \ . \ s \quad s' \end{matrix}\right\} \text{ or, if } s'=0, \left\{\begin{matrix} D \ . \ . \ . \ s \quad 0 \\ \qquad\qquad \times \\ D' \ . \ . \ . \ s \quad 0 \end{matrix}\right\};$$

where the cross-lines indicate the direction of change which the sounds take in assuming the interchanged forms of D'.

I propose therefore, for convenience, to designate the process so formulated "Cross Compensation". It is perhaps more expressive to the eye, however, to formulate it thus :—

$$\begin{Bmatrix} D & \ldots & s & s' \\ D' & \ldots & s' & s \end{Bmatrix} \text{ or } \begin{Bmatrix} D & \ldots & s & 0 \\ D' & \ldots & 0 & s \end{Bmatrix},$$

the correspondent sounds being coupled by points.

(*d*) A favourite example of Cross Compensation is furnished by the deaspiration and the compensatory aspiration of the thorough-bred Cockney, and others. As Curtius himself remarks, in his chapter on Irregular Aspiration[1], it is, apparently, a law of language that when the *spiritus asper* enters on a course of debilitation, it has a tendency, on the other hand, to thrust itself in where it has no business. Apart from the figurative language which represents sounds as doing so and so, this is undoubtedly true. People who at one moment talk about the *'air* of the *'ead* almost invariably terrify us the next moment with the truly Gorgonian *h*air of the *h*atmosphere: such vagaries form an inexhaustible mine of bad jokes for our comic journals—just as, long ago, they furnished Catullus with matter for an epigram[2]. There is, in modern times, with the rapid march of education, some chance of reducing these irregularities to rule; but most languages, even Greek and Latin, embody some words which, in times of general ignorance, have either dropped or improperly assumed the breathing, and have worked their way up into the standard dialect.

(*e*) Another example of Cross Compensation is supplied by G. P. Marsh[3]. "It consists in suppressing the *r* where it should be heard, and adding it where it should not. One need not go a day's journey from New York to find educated persons who call the municipal rule of action the *lor*, and yet style the passage from one room to another *a doah*." Numerous similarly correlated pairs of words might be collected in London, but certainly *not* among educated persons. *Apud*

[1] Grk Et., p. 671. [2] Carmen lxxxiv. (De Arrio).
[3] Lecture xxx., Smith's edition.

plebem, indeed, while the *r* is rarely heard when it should be, especially as a final, on the other hand, nearly all words ending in open vowel-sounds are liable to the trill (*e. g.* dro*r*ing, follo*r*ing, swallo*r*ing, fell*er*, Jemim*er*, extr*er*, "*and*-ceter*er*," instead of dra*w*ing, follo*w*ing, swallo*w*ing, fello*w*, Jemim*a*, et-cetera). Both this and the foregoing example are (or at least were) prevalent characteristics of the popular Cockney dialect; and both fall under the first variety of Cross Compensation; for *h* and the Cockney *r* have both reached the last degree of debilitation. These two examples therefore may be represented by the following particular cases of the general formula:—

$$\begin{Bmatrix} D & \ldots & h & 0 \\ D' & \ldots & h & 0 \end{Bmatrix} \text{ or } \begin{Bmatrix} h & 0 \\ \vdots & \vdots \\ 0 & h \end{Bmatrix} \text{ and } \begin{Bmatrix} r & 0 \\ r & 0 \end{Bmatrix} \text{ or } \begin{Bmatrix} r & 0 \\ \vdots & \vdots \\ 0 & r \end{Bmatrix}.$$

(*f*) But we are not without excellent examples of the second variety; and these also appear among the lower strata of the population in London. Thus the gutturo-nasal, represented by *ng*, is, or was[1], in the terminations of participles and verbal nouns, almost universally softened to the dento-nasal *n*,—as *obligin'*, *screamin'*, *a-goin'*, *affectin'*, *hollerin'* (hallooing), a *farden* (farthing), a *pudden* (pudding), &c., &c.; while, on the other hand, *ng* for *n* has (or had) permanently established itself in certain words,—as *capting*, *certingly*, *cousing*, *curtings*, *fashings*, *kitching*, *garding*, *mounting*, *musling*, *sovering*, "beggin' yer parding, sir"—for captain, certainly, cousin, curtains, fashions, kitchen, garden, mountain, &c. But perhaps the most remarkable case of all is the well-known and once general interchange between *v* and *w*. This is an example of the very highest importance, in which, however, if I mistake not, the apparent Cross Compensation is not really such. As I shall presently have to treat of it as suggesting

[1] I say "*is* or *was*," on account of the rapid spread of education and the increased intercourse between the overlapping sections of society; an inevitable consequence of which is the reduction, or approximate reduction, of spoken language to a dead uniformity wherein no "horrible vulgarisms" (*i. e.* incipient or obsolescent dialectic forms) are allowed to appear, unless they have been promoted at least to the rank of slang.

a second approximation to the *rationale* of Grimm's Law, I shall content myself, at present, with formulating this and the preceding example:—

$$\left\{\begin{matrix} \text{D} \ . \ . \ . & ng & n \\ & \times & \\ \text{D}' \ . \ . \ . & ng & n \end{matrix}\right\} \text{ or } \left\{\begin{matrix} ng & n \\ \vdots & \vdots \\ n & ng \end{matrix}\right\} \text{ and } \left\{\begin{matrix} w & v \\ \times & \\ w & v \end{matrix}\right\} \text{ or } \left\{\begin{matrix} w & v \\ \vdots & \vdots \\ v & w \end{matrix}\right\}$$

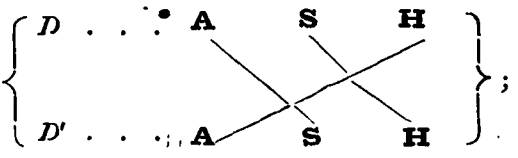
19.—(*a*) In attempting to examine the philosophy of this curious process, the first feature that strikes us is its remarkable *correlative duality*. No example has come to my notice which does not exhibit this characteristic; nor is it easy to imagine how a third sound can be involved. Known facts therefore prove that, sometimes, when in any dialect of a language a sound is disappearing, by debilitation, from its proper place, there is a tendency by a counterbalancing corruption, to insert it where it was not previously found; or, when one sound is, by debilitation, substituted for a second, there is a tendency, by a counterbalancing corruption, to substitute the second for the first. I use the qualifying words "sometimes" and "a *tendency* to substitute and insert," because it is manifest that in the overwhelming majority of debilitations (take *e.g.* the extensive weakening of the Latin tenues to mediæ in Spanish) not the slightest compensatory *nisus* is to be traced, and because it is by no means certain that compensation, where it *does* operate, is perfectly coextensive with its correlative debilitation. Both of these points will receive elucidation in the ensuing sections. I pass on now to observe, secondly, that the dual movements constituting Cross Compensation are always and necessarily *contemporaneous*; thirdly, that the dialect in which they exhibit themselves is always and necessarily *in presence* of that from which it is varying; and, once more, that the compensatory sounds are always the *exact equivalents* of those which they so irregularly supply.

(*b*) I indicate these features here with all brevity, partly because I am convinced that the more carefully any reader chooses to study the process, the more readily will he admit their accuracy, and partly because I am anxious to dismiss

for good the theory which has now so long occupied us. If the Compensation described by Curtius be compared with that actually observable, it will be seen that the two processes have not a single feature in common. In truth, the former can hardly be called Compensation at all: for if **A** became **S**, **S** ought to have become **A**,—which it did not; and if **S** became **H**, **H** ought to have become **S**,—which it likewise did not. Instead of legitimate compensation we have the anomalous groups—

$$\left\{\begin{matrix} D & . & . & \mathbf{A} & \mathbf{S} \\ & & & \vdots & \vdots \\ D' & . & . & \mathbf{S} & \mathbf{H} \end{matrix}\right\} \text{and} \left\{\begin{matrix} \mathbf{S} & \mathbf{H} \\ \vdots & \vdots \\ \mathbf{H} & \mathbf{A} \end{matrix}\right\};$$

(where *D* now represents the Cl., and *D'* the H. G. dialect,) or, marking by lines the course of debilitation,



all which is to be repeated for the so-called second *Verschiebung*,—the whole constituting a series of interchanges for which we may fairly ask some further explanation than the reiterated assertion that "the new media *verschob* or *trieb* the old one" out of its place, and so on. I say nothing here of the assumption (inseparable from any form of the Chronological Hypothesis) that D' was no longer in presence of D from which it was thus differentiating itself—whence it follows that the several movements are necessarily represented as successive, and not contemporaneous, although the contrary condition is *the* fundamental one of genuine Cross Compensation. Enough, in sum, if it be seen that the reference to Compensation is scarcely more fortunate than the reference to Discrimination, and that, including the dubious character of the initial substitution, the whole theory, as an explanation of Grimm's Law, is one which itself requires to be, from beginning to end, explained.

20.—(*a*) The precise nature of Cross Compensation has, however, in some of its aspects, been clearly perceived by one of our leading Teutonic scholars (Henry Sweet, Esq.); who, in an Essay from which the passage quoted below[1] is taken, has made an ingenious attempt to apply the process to explain the peculiarities of the German phonetic systems. But superimposing this application upon the Chronological Hypothesis, he necessarily attains results from which I am unfortunately compelled to dissent. To avoid diverging, at this point, too far from our direct line of inquiry, I propose to devote an Appendix (D) to a somewhat full statement and discussion of this subsidiary hypothesis. I will at present refer to it only so far as it affords an opportunity of further elucidating the process under consideration. Its main feature is, that, instead of evolving Σ_y directly from Σ_x (and, of course, Σ_z from Σ_y) by a single phonetic movement, involving all three mutes, it divides each evolution into two successive movements, each movement involving two mutes. Thus, putting (as in § 2)

$$\Sigma_x = \mathbf{H} \quad \mathbf{A} \quad \mathbf{S},$$

the first stage towards Σ_y (which we will designate $(\Sigma_{x'})$) is supposed to be reached by interchange between **H** and **S**, **A** remaining constant:—

$$\begin{array}{rcccc} \Sigma_x = & \mathbf{H} & \mathbf{A} & \mathbf{S}, \\ & \vdots & & \vdots \\ (\Sigma_{x'}) = & \mathbf{S} & \mathbf{A} & \mathbf{H}. \end{array}$$

[1] "The oldest changes of *t* into *d*, and *d* into *t* must have occurred simultaneously; otherwise the original *t* and *d* would have merged into one sound, either *t* or *d*, without the possibility of an after restoration of the original distinction. The phenomenon is in fact a case of simple confusion or interchange, as familiarly exemplified in the vulgar *hair* for *air* and *'are* for *hare*, when heard, as is not unfrequently the case, from the same mouth.—It is important to observe that such changes are quite independent of general phonetic laws, and, as in the present case, as often directly opposed to them; for if the change from *t* to *d* be a weakening, the other from *d* to *t* must be a strengthening, and therefore opposed to the general tendency of sounds."—Appendix to Gregory's Pastoral Care, p. 498.

To reach the next stage, **H** is supposed to remain constant, while interchange takes place between **A** and **S**:—

$$(\Sigma_{x'}) = \begin{matrix} \mathbf{S} & \mathbf{A} & \mathbf{H}, \\ \vdots & \vdots & \\ \mathbf{A} & \mathbf{S} & \mathbf{H}. \end{matrix}$$

And Σ_x is supposed to be evolved from Σ_y, through an intermediate stage $(\Sigma_{y'})$, in a precisely similar way.

(*b*) Now it will probably be a mystery to any reader who has duly weighed the natural rigidity of the mutes, why these sounds should, among the German tribes above all others, be supposed to have undergone, for no apparent reason and with no apparent advantage, such a succession of violent changes. The changes assumed by the Chronological Hypothesis pure and simple are sufficiently numerous and inexplicable; but this variation of that hypothesis largely increases the sum-total of fluctuation. Instead of one primitive system and *two* derived systems, we must here imagine one primitive system and *four* derived systems; while, in the evolution of each of the German systems in the way supposed, one of the mutes must be represented as undergoing change twice over. All this is inconsistent with the behaviour of the mutes in all other parts of the world, and, indeed, among the Germans themselves during historic times.

(*c*) Nevertheless, if the fundamental hypothesis were true, if successive evolution were possible, this extension would offer an undoubted improvement on Curtius's scheme of Compensation. Every subsidiary hypothesis, however, is necessarily affected by the defects of that to which it is attached. The former may involve principles all of which are, under given conditions, demonstrably true; yet in its new relationship some of the essential conditions and phenomena may cease to exist. Of this I think we have before us a case in point. The principle of Cross Compensation, under given conditions, is true beyond question: some of its phenomena—*e.g.* the correlative duality and the simultaneity of its counterbalancing movements—are distinctly acknowledged by Mr. Sweet. But others, although equally

vital, being incompatible with the fundamental hypothesis, are necessarily ignored. It will be pointed out, for example, in Appendix D, that some of the counterbalancing compensations are by no means exact equivalents (as they ought to be) of the sounds from which the correlative sounds fell away. Further, although $(\Sigma_{x'})$ and $(\Sigma_{y'})$ should have once existed as the standard systems against which Σ_y and Σ_z differentiated themselves, yet no trace of their existence is discoverable. What has become of them? Are we to suppose that the subordinate and fluctuating absorbed the standard and fixed systems? This is contrary to observation; and if it were not, the supposition would still involve some perplexing incongruities. For then, among the Germans, $(\Sigma_{x'})$ must have *completely* absorbed Σ_x (which, by the hypothesis, they brought with them from Asia), and Σ_y must have completely absorbed $(\Sigma_{x'})$; whereas $(\Sigma_{y'})$ did *not* absorb Σ_y at all, but was itself absorbed in Σ_z. Lastly, it will be shown in Appendix D that the Σ_z of the Chronological Hypothesis does not, after all, exhibit the form which such a mode of evolution should produce.

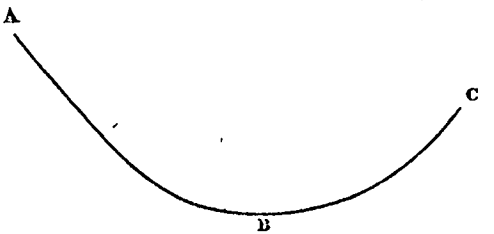
21.—As the points just discussed affect the very essence of Cross Compensation, and as this process is of great importance to my own inquiry, I ask leave to devote a few lines to a further consideration of them and it. And first I observe that in Cross Compensation we cannot remove one of the combined dual movements from the other. The leading movement in the whole process is the reduction of a stronger sound to a weaker, and therefore falls under the Principle of Least Effort; while the counter movement is directly educed by the initial one acting under the special condition of dialectic contact or commixture. Numerous examples on a large scale (the Romance languages to wit) teach us that when sound-change is running through a whole dialect or language *separated* from its related dialects or languages, then we find but a single and uniform phonetic movement all tending in one direction, *i. e.* towards debilitation; nor can we conceive how or why

it should be otherwise. But if a dialect in which the debilitation of one (*s*) of two related sounds into the other (*s'*) is in progress continues in presence of a related dialect which actively and of set intention resists such debilitation, or which is at any rate passively stable, then in the minds of those among whom the debilitation is gradually prevailing there just as gradually grows up an uneasy sensation, a dim consciousness, that something is defective or amiss in their own phonetic machinery; for where they employ *s'*, they hear others about them continually employing *s*. An assimilation of their mental condition by education, or other means, to that of the contiguous stratum of society would enable them to apply the proper remedy by restoring *s'* to *s*; whereas, acting under the influence of a vague impression, instead of trained observation, they believe they are applying a remedy when they proceed to employ *s* where they hear *s'* in the surrounding dialect. In point of fact, the habit, into which they fall, of continually substituting *s'* for the *s* which they as continually hear about them, induces in their mind what I shall venture to call a "Dissimilating Sentiment". To this, when fully developed, they would actually be doing violence if they were to reassimilate *s'* to *s*, whereas by further substituting *s* for *s'* they even gratify it. And in this process they are assisted by the principle of Analogy; by which is meant the tendency of men, when they have set a number of "leading cases," to adjust all others to the same pattern and precedent[1]—that is, to reduce irregularity to regularity. In a word, men exhibit in the case of language (only less consciously) metaphysical phenomena akin to those exhibited by them in coexisting political or social parties, and even schools of thought. Action and innovation excite counteraction and resistance; insomuch that when sides have been finally taken, the mere fact that a plan or a doctrine proceeds from one side often excites opposition on the other, even though the subject of opposition is one

[1] See the concluding Chapter in Mr. Sayce's "Principles of Philology"; or Schleicher, "Die D. Spr.", p. 61 *et alibi*.

which, on its own merits and apart from its parentage, its opponents would just as readily entertain.

22.—(*a*) It can hardly be said, therefore, that the changes involved in Cross Compensation are "quite independent of general phonetic laws" even when "often directly opposed to them". If the raising of *s'* to *s* were a single and detached movement, such a statement might hold good; but it is not. Although, therefore, when viewed alone it appears an exception to ordinary laws, yet viewed in connexion with its exciting movement it becomes an exception that proves the rule; for, as Max Müller admirably remarks, "as in other sciences, so in the science of language, a law is not violated, on the contrary it is confirmed, by exceptions of which a rational explanation can be given"[1]. In the case before us the apparently abnormal process is not only susceptible of a "rational explanation"; it is a direct result of the operation, under given conditions, of the universal Principle of Least Effort. If I may borrow a physical illustration, I will refer to the favourite Russian amusement of the "flying bridge", in which a sledge, starting as from A, slides by its own weight



down a curved decline, as AB, to B, and by the impetus so acquired is carried up the incline BC to C. Here it would hardly be fair, in estimating the entire movement, to separate the incline from the decline, and to represent the motion of the sledge up the former as "quite independent" of the action of gravity; for this motion is the immediate result of such action under the conditions imposed by the figure of the path.

(*b*) Without riding our illustration to death, we may per-

[1] Lectures, ii. 237.

haps give it a further application; for, from causes we need not specify, the net result of the whole movement from A to C is a considerable balance in favour of the downward motion due to gravity; and similarly the net result of the whole phonetic movement constituting Cross Compensation generally shows a preponderance on the side of debilitation. Such is certainly the case with respect to the *h*, *r*, and *-ng* considered in § 18—although it must be admitted that, at least in the last two instances, the number of examples to which the compensatory strengthening is applicable are far fewer than those which are susceptible of the correlative debilitation. With respect to the *h*, I have frequently observed a fact of some interest; viz., that when a group of deaspirators are talking together, an *h* is rarely heard at all; but when any of them begin to talk with people of superior education, they also begin to adorn their conversation with a frantic sprinkling of *h*'s in the wrong places, in order (as they suppose) to adapt their language to their listeners[1]. With the spread of education it becomes increasingly difficult to observe such facts; but if this one were established by a sufficient number of observations, it would offer valuable subsidiary testimony to the dissimilating effect produced by one dialect upon another in contact with it.

(*c*) I will conclude this section with a *résumé* of all the conditions and phenomena of Cross Compensation that have now come under our notice. (1) It originates in debilitation of the stronger of two sounds, by one of two commingled dialects, in strict conformity with the Principle of Least Effort. (2) The fluctuating dialect in which such debilitation sets in must be in presence of a resisting (and in this respect standard and fixed) dialect, which maintains the stronger sound. (3) The

[1] The ludicrous effect is often heightened by the air of self-satisfaction which proclaims that the speaker in his or her own opinion has evidently done a fine thing, as who should say, "You see we can speak as elegantly as you, if we like." Catullus therefore, in the epigram before referred to, no doubt copied nature closely in his description of Arrius,—

> [qui] "tum *mirifice* sperabat se esse locutum,
> quum, quantum poterat, dixerat *h*insidias."

resisting dialect acts on the former—*i. e.* on those who speak it—by evoking (*a*) an obscure perception of phonetic discrepancy, (*b*) an instinctive Dissimilating Sentiment[1], (*c*) an effort to remove the discrepancy; (*d*) which effort, however, is diverted by the said sentiment into a counterbalancing discrepancy. These fundamental conditions involve the minor phenomena stated in § 19 (*a*), viz.:—(4) the correlative duality of the phonetic movements; (5) their contemporaneity; and (6) the identical phonetic value of the interchanged sounds, both of which, previously to the movement, are common to both dialects.

(*d*) I have purposely employed much time and some repetition in elucidating the characteristics of this important process—firstly, because I do not know that the same thing has anywhere else been done, and, secondly, because the process itself is, on a small scale, a genuine *Lautverschiebung* under the only known conditions which admit thereof. Some of its phenomena, indeed, do not directly illustrate the principles on which, as I believe, a correct theory of Grimm's Law is to be based; but others exhibit language as acting with a systematic vigour even greater than such a theory postulates. We shall hereafter have to inquire what results follow from absence or modification of any of those conditions. Our business up to the present has been to track out various hypotheses on the lines laid down by their authors; to examine the forms of compensation suggested to us; to construct the forms actually

[1] No reader will, I hope, for a moment imagine that this Dissimilating Sentiment is but the reproduction, under another name, of the Discriminating Impulse whose existence is questioned in §§ 16, 17. The former represents a fair induction from observed facts, and is really a *habit of mind* generated by the differences between dialects in contact. The latter is a mere creation of the imagination, and is actually opposed to the habit of mind which should have been generated by the conditions under which the Chronological Hypothesis supposes the evolution of Σ_y from Σ_x to have taken place. For, firstly, the L.G. dialect is supposed to have been completely removed from the influence of the Cl. dialect; so that there seems to have been no agency for evoking the sense of phonetic discrepancy; and, secondly, as the first movement of the whole evolution was the fusion of one series of mutes with another, the sentiment generated by that movement should rather have been an *Assimilating* Sentiment.

warranted by facts; and to show that the conditions involved in those hypotheses are at variance with the results at which we have arrived.

23.—(*a*) We have now reached a point at which we may fairly stop to breathe and look around us. So far we have found that no explanation of Grimm's Law which takes the Historical or Chronological Hypothesis as its groundwork is altogether satisfactory. We may have started fairly enough; but, whichever road we have taken, we have sooner or later found a lion in the way. There is therefore some ground for supposing that the discordance between that hypothesis and the conclusion from the Principle of Sufficient Reason (§ 3) may be entirely the fault of the former; so that we shall be all the better prepared to admit that that conclusion may be a truth the neglect of which surely leads to the contravention of other truths more directly demonstrable, and to suspect that no hypothesis of Grimm's Law can lead to satisfactory results which is based on the *successive* evolution of the three phonetic systems Σ_x, Σ_y, Σ_z, or (what is the same thing) which does not take their contemporaneous evolution for a fundamental principle. Thus we return to the point from which we diverged (§ 4) into an examination of various aspects of the Chronological Hypothesis.

(*b*) One philologist of the first rank (Prof. Max Müller) long ago endeavoured, as we have seen (§ 3), to enforce the view to which we now recur. But the line of reasoning (if it was not rather the intuition of a keen and practised intellect) by which he reached it, does not stand out in his Lectures with that fulness and clearness which generally characterize the writer. It is partly for this reason perhaps that the reception accorded to his statement of the view has scarcely answered to the Professor's great reputation; but it is partly also, I think, because there appears to be some inconsistency between the different parts of that statement. For as Baudry[1] has pointed out, these really constitute two separate hypotheses. The first properly belongs to the class of hypotheses which we

[1] G.C., pp. 144, 146.

have already examined and put aside. For although it represents the substitutions as going on contemporaneously in Σ_x, Σ_y, and Σ_z, yet it not only assumes the pre-existence of an earlier triple mute-system, but it also represents the individual sounds of Σ_x, Σ_y, Σ_z, as being *successively* adjusted to their actual place in those systems; and the order of adjustment in Σ_y is precisely that adopted by Curtius[1]. The ultimate cause of the whole movement is stated to be this,—that "the Teutonic tribes, in taking possession of the phonetic inheritance of their Aryan, not Indian, forefathers" (as if this were something in which they had previously had neither part nor lot), "retained the consciousness of the threefold variety of their consonantal checks, and they tried to meet this three-fold claim as best they could"[2]. The first substitution (**S** for **A**) of each of the German systems is represented to have been a matter of preference; the others, of necessity. Under the sway of "a wish to keep distinct what *must be* kept distinct," the old Soft-mutes and the new "*could not be allowed* to run together;" and so the old ones were raised to Tenues. Then arose "pressure" and "necessity" which *drove* the Gothic nations to the third substitution;—with much more of the same kind: all which is but a variation of Curtius's assertion that "Das junge *d verschob* das alte *d*," &c.

(*c*) This aspect of the hypothesis therefore represents only a perplexing compound of the simultaneous and the successive—of inherent phonetic necessity (pressing, be it remembered, upon the Germans alone), and, apparently, of arbitrary contrivance and convention,—which lies open to most of the objections applicable to the purely chronological hypothesis, and to others besides. These we need not here produce or reproduce; for the learned author presently broaches his hypothesis under quite another aspect:—"From the very beginning different branches of the Aryan family fixed the three cardinal points of the common phonetic horizon differently. While the Hindus fixed their East on the *gh*, *dh*, and *bh*, the Low Germans fixed it on the *g*, *d*, and *b*, the High

[1] See §§ 11, 12, *antè*. [2] Lectures, ii. 225, 226.

Germans on *k, t, p*. All the rest was only a question of what the French call *s'orienter*"[1]. This statement is intimately connected with a previous one, which refers to a condition of things anterior (it would seem) to "the very beginning":—"I feel strongly inclined to ascribe the phonetic diversity which we observe between Sanskrit, Greek, and Latin, to a previous state of language, in which, as in the Polynesian dialects, the two or three principal points of consonantal contact were not yet felt as definitely separated from each other"[2]. And again:—"I can conceive different definite sounds arising out of one indefinite sound; and those who have visited the Polynesian islands describe the fact as taking place at the present day. What then takes place to-day can have taken place thousands of years ago; and if we see the same word beginning in Sanskrit, Greek, and Latin, with *k, t,* or *p*, it would be sheer timidity to shrink from the conclusion that there was a time in which that word was pronounced less distinctly; in short, in the same manner as the *k* and *t* in Hawaian"[3].

24.—This view, however, seems to me to go too far in the opposite direction—farther than any known facts will warrant. It means, if I understand it aright, that not merely the three related sounds of each family, but also the three families themselves—*i. e.* no less than nine sounds—were all evolved out of one primitive "indefinite" sound. Curtius may well ask[4] what that protean and indescribable sound could have been. But it is still more to the point to remark, as he further does, that (not indefiniteness but) vigour and distinctness must have been, so far as we can judge, the characteristic of the Holethnic phonetic system[5]. In the most ancient remains of the separate dialects, the distinction between the different families of mutes is quite

[1] Lectures, ii. 231.
[2] Id., p. 197.
[3] Id., p. 200.
[4] Grk Et., p. 412.
[5] Max Müller's illustration from the Hawaian is of dubious application. The confusion there existing is far more like a late corruption of an earlier and purer condition of the mutes, than an embodiment of the primitive state of the language.

as sharply marked as in the younger dialects, or even more so; while the conditions regulating the transition of a primitive *k* into *t*, *p*, and still feebler sounds, have been pretty clearly ascertained, and are almost certainly independent of any phonetic instability in the Holethnic period[1]. Whatever may have been "indistinct," the lines of demarcation between *k*, *t*, and *p*, in the period immediately preceding the so-called *Lautverschiebung*, were certainly not so. Nor, so far as the most ancient remains of language enable us to judge, was there any confusion, within historical memory, between the much more closely related sounds forming the three mutes of each family. Nevertheless, as the members of each family are produced at one and the same point of the vocal passage under but slightly differing conditions, it is, on physiological grounds, not improbable that all three of each group may at a still remoter period have radiated from a single sound; so that, if we may make Max Müller's suggestion refer to each family only[2], I believe we shall find in it a close approximation to one of the fundamental truths on which, as I make bold to think, a correct theory of Grimm's Law must be based. But in descending from the general statement to the working out of its details, we shall be compelled at once to part company with the learned Professor. For if, without going beyond his own hypothesis, we test his suggestion by the ruling Principle of Least Effort, we shall at once perceive the necessity of an important modification; for a single sound that should generate three related sounds

[1] See §§ 55–63 *infrà*. I am not denying that future investigators may find some justification for referring all three families to a single primordial mute. But if they do, I venture to predict that that mute will not be a flabby and "indefinite" one, but the hardest and firmest of them all, —*i. e.*, K (unless a firmer should yet be discovered).

[2] G. Michaelis ("Ueber den Unterschied der Consonantes tenues und mediæ," &c.), while thus limiting the suggestion, finds the parent sound of each family in the Sanskrit Soft Aspirate, which seems to him a sort of chaotic jumble offering no clear distinction between articulation, breath, and voice. C. Arendt, in reviewing the tract (Kuhn's Z-S., xii. 441), anxiously (and, I think, successfully) defends those sounds against such an imputation.

differing in strength could not be weaker than the strongest of the three, or else, by that principle, it could not have given off the strongest. We are unable, however, not only to produce but even to conceive of any sound or sounds stronger or purer than the Tenues; and as the other sounds to be accounted for are weaker or less pure than their related Tenues, and could be directly derived from them[1], what ground or what need is there for assuming the existence of parent sounds for the three families differing in any respect from the three Tenues themselves?

25.—(*a*) I propose then, that, instead of some unknown, "indistinct", and undefinable sound, we take the purest, the strongest, the most distinct sound of each of the three families as the primordial one of its family, and as the parent of the other two. By this means we shall, I think, at once put ourselves in harmony both with the Principle of Least Effort, and with the acknowledged vigour and distinctness of the earliest I-E. mutes. What is more, we shall effect a junction with the conclusion from Sufficient Reason, by which we are forbidden (§§ 2, 3) to ascribe priority or superiority of any kind, to one of the three leading phonetic systems over the remaining two. For it is certain that the symmetrical ter-trinal trinity constituted by those three systems together cannot have existed from all time. Indeed the very object of every inquiry into Grimm's Law is to trace all three to some single primitive system. But if no one of the three may be deduced from either of the others, we must assume that Σ_x, Σ_y, and Σ_z were evolved within some fixed period from some pre-existent system (let us call it Σ_1), which must have *differed* both from Σ_x, Σ_y, and Σ_z (or else that one of the three which coincided with it would really be Σ_1, and therefore prior to the others, which is contrary to the hypothesis), and yet must have been *symmetrically related* to them all, and they to it; for each of them simultaneously or contemporaneously derived from it precisely the same sounds as the others, and in such a way that to the same

[1] See Appendix B.

sound in each there always corresponded the same sounds in the other two: that is to say,

Where Σ_x derived **H**, Σ_y derived **A**, and Σ_z derived **S**;
„ Σ_y „ **H**, Σ_z „ **A**, „ Σ_x „ **S**;
„ Σ_z „ **H**, Σ_x „ **A**, „ Σ_y „ **S**.

(*b*) This tabulation may perhaps enable us to discover what that symmetrical relationship must have been; for, taking the totals of the three vertical columns, we observe that all the three systems together derived from Σ_1 three complete and coextensive series of mutes—one of one degree (**H**), another of another (**A**), and another of the third (**S**)—each comprising as many sounds as each of the three fragmentary series (**H A S**, or **A S H**, or **S H A**) which severally make up Σ_x, Σ_y, and Σ_z[1]; or, as the same fact may be stated, Σ_1 was of such a nature as to give off, or allow to be given off, for distribution among Σ_x, Σ_y, Σ_z, three coextensive series of mutes—one consisting of Tenues only, another of Aspiratæ only, and the third of Mediæ only. Σ_1 therefore must have comprised either *three* perfectly coextensive series of mutes (*i. e.*, $\Sigma_1 = \Sigma_x + \Sigma_y + \Sigma_z$), or else only *one* uniform series[2], from which the other mutes must have been derived. Now to suppose (as nobody, however, can) that such a triplicate series of mutes ever existed would really be to suppose that the vocabulary of the older Holethnic speech was tripled in that part (and that only) in which the mutes were involved; so that every mute-built word would and did, in one and the same language, appear in three precisely equivalent forms constructed by merely changing its consonants. This supposition may be at once dismissed on the ground of its own inherent absurdity. The alternative is that the older system comprised only one[3]

[1] The symbols Σ_x &c. have hitherto indicated the *arrangement* of the three classes of mutes in the dialects to which they severally appertain; their meaning is now extended to denote the *whole series* (or quantity) *of mutes* in those dialects.

[2] See the following note.

[3] Σ_1 cannot represent *two* such series of mutes; for as the numbers 2 and 3 are prime to each other, Σ_1 could not in this case have stood in any symmetrical relationship to the younger systems either singly or together. It

of those uniform and coextensive series, and that the triform series constituting each of the younger systems resulted from a twofold variation affecting different sections of that one older series, but leaving a third section unaffected,—the sections altered and the sections retained by the several systems being reciprocally adjusted among those systems upon a plan which will presently, I hope, appear to be not altogether inexplicable. But when we speak of the "variation" of articulate sounds, we are compelled by the Principle of Least Effort (unless some counteracting principle is shown to be in action) to understand "debilitation." Consequently we have to assume that the one unbroken parent series was made up of mutes of that degree which is susceptible of debilitation into mutes of the other two degrees. Which of the three degrees that must have been, admits of little discussion. The Mediæ, by common consent, are out of the field. The Aspiratæ are held, even by those who seem to teach that they are stronger than the Tenues, to result from an "affection" of the latter[1]; while the latter are never represented as derived by natural debilitation from the former. The three Tenues therefore alone remain as the primordial mutes, beside which there were originally no others, and from which all the others, at a later period, contemporaneously radiated by systematic debilitation[2].

26.—The fundamental proposition, then, on which alone it seems to me possible to build a satisfactory theory of Grimm's Law is, that the two weaker degrees of mutes were evolved from

would have partially coincided with some one of them; and the evolution of the others would be even more difficult to account for than on the ordinary Chronological Hypothesis. Besides, it would still imply the existence of a duplicate vocabulary; and the objection to this differs only in degree from the objection to a triple vocabulary.

[1] See Appendix B.

[2] The line of argument intended to lead up to the sole existence (*i. e.* for linguistic purposes) of the strongest sound of each of the three families, is of course to be kept distinct from the claim of the three Tenues to be those sounds. The former will hold good, even though this claim should ever be disputed.

the remaining stronger mute of each family, which stronger mute could scarcely have been any other than the Tenuis. This proposition is of the very highest philological importance, and may be exhibited under several different aspects. It means, for example, that there was once a time in the history of the Indo-Europeans when the Tenues were the only mutes appropriated to the service of speech[1]. It means either that once on a time all the Aspiratæ and all the Mediæ of the several I-E. Mute-systems were actually Tenues; or, at any rate, that the words in which Aspiratæ and Mediæ occur radiated by phonetic variation from words involving Tenues only. And it means that the Tenues of each system are what that system

[1] This reduces the Holethnic (I-E.) speech, so far as the mutes are concerned, precisely to the ancient condition of the Finnish, and supplies substance for the rather shadowy arguments of Förstemann (Gesch., i. 236–238) in favour of a primeval connexion between the two families of speech. The proposition of the text is analogous to the important doctrine (§ 6 (*a*) *antè*) that there was once a time when our forefathers employed no other vowel than *a*. The bearing of such doctrines, when demonstrated, upon the problem of the Origin of Language, or at least of our great family of languages, will be manifest at a glance. For when that problem is reduced to the question, how a single vowel, two or three consonants, and perhaps three or four other sounds—in all not more than eight (which number may yet be diminished)—how these, I say, were originally appropriated to the expression of ideas, a complete solution is surely not to be despaired of. It is in vain for learned bodies (like the Parisian *Société de Linguistique*) to refuse to listen to inquiries on this subject. The problem is one which, until solved, will always possess an irresistible fascination for speculative thinkers; but it is also one that can never be solved by mere speculation. What we want to know is, not how language *might* or *should* have originated, but how it *did* originate. We want a lantern for our feet, not fireworks about our head. We must advance towards the solution by steps which rest on solid facts, even though at each step it cost much labour to plant the foot firmly; and such a step is clearly gained every time sufficient reason can be shown why the phonetic materials of speech should be simplified and diminished in number.—I need hardly add that in speaking (in the text above) of language as "appropriating" various sounds to its own uses, I do not mean to hint either that such sounds previously had an independent existence, or that no others were producible by the vocal organs of man. A few, very few, sounds of broadly marked character must, in the very infancy of our race, have been, one after the other, thus directly appropriated; the rest were evolved from these by phonetic variation.

preserved for itself out of the earlier and larger common stock of Tenues; so that the L.G. Tenues are not substitutes for the Cl. Mediæ, nor the H.G. Tenues for the L.G. Mediæ; but the Tenues of all three systems are of equal antiquity and coordinate primitiveness, and constitute, when put all together, the ancient common stock, of which the several tribes subsequently allowed different parts to be differently weakened. And by this general weakening the evolution of all the systems is brought into harmony with the Principle of Least Effort; so that we have no longer to provide, at successive stages, for the raising of **S** to **H**, nor even for the doubtful change from **A** to **S**: we have only to assume the debilitation of one stronger sound in two diverse directions; which twofold debilitation must, as has now been so often insisted upon, have gone on simultaneously in all the three dialects.

27.—(*a*) The leading positive conclusions which we have now arrived at are, in sum, that the three systems, Σ_x, Σ_y, and Σ_z are of coequal antiquity and coordinate importance, and that they were contemporaneously evolved from a single preexistent system comprising Tenues only. I shall next try to show what light is thrown by these conclusions, and by our collateral inquiries, upon various points which the Chronological Hypothesis left, to say the least, in obscurity. The chief of such points are (1) the cause (or agency) and the object (or purpose) of the assumed changes of the mutes; (2) the dialectic place of origin of those changes; and (3) their symmetrical distribution among the principal tribes of the Holethnos.

(*b*) As to the first point, we have seen not only that, on the hypothesis just mentioned, no satisfactory explanation of the individual Mute-changes is forthcoming, but also that the whole movement described as the first *Verschiebung*, and its assumed repetition in the second, are both, to all appearance, absolutely wanton. Each is a mere revolution, quite incomprehensible in a series of sounds which are supposed to have been native to, and amply sufficient for, the tribes among whom the movement is supposed to have been effected; and each exhibits phonetic changes whose possibility is more than

doubtful. But if the conclusion of § 25 is laid down as a fundamental doctrine, the whole state of the case is immediately and completely altered. The effective agent of all the changes is identified with the ruling principle of debilitation under which they, as sound-changes, ought naturally to fall; and their object becomes the enrichment of language by the multiplication of its phonetic resources: for whereas there had previously been but *three* mutes (viz. the Tenuis of each family), there were now *nine*; that is to say, phonetic provision was made for *trebling* the expressive *capacity* of a large section of the Holethnic vocabulary. Such a provision is one which the linguistic sense would immediately avail itself of; and, in fact, it is just here, if I mistake not, that that species of Differentiation described in § 17 (*d*) found an ample field of action. If, with the constant acquisition of new ideas and subdivision of old ones, the alphabet had remained fixed within its original narrow limits, one and the same verbal form would soon, in many cases, have had to do duty for a continually increasing number of meanings; so that at last the parent speech would have become crowded with clusters of homonyms[1]. From

[1] Baudry, who makes light of the supposed evil of homonyms in a modern language (G.C., p. 145, quoted in note to § 17 *ante*), treats it (p. 146) as a very serious matter in the parent tongue. "La confusion primitive entre les aspirées et les pures" (he is combating the second aspect of Max Müller's hypothesis) "est une conjecture qui paraît peu admissible, à cause des homonymies nombreuses qu'elle aurait créées. Les homonymes par corruption se conçoivent et sont inévitables; mais à l'origine, il semble qu'ils auraient tout brouillé." As nothing is stronger than its weakest part, I might content myself with denying the "il semble" of the foregoing quotation. But as the objection, if there were anything in it, would tell with much greater force against the theory of the text above than against Max Müller's (for there can be no mistake about the unity of the Tenuis; whereas Max Müller's uncommonly "indistinct" sound might be all things to all men,—one and yet many at the same time), I will endeavour to do for my denial what Baudry does not do for his assertion—*i. e.* give some reason for it. The objection, then, I think, implies that all the words which in all the I-E. dialects at or just before the Separation exhibited Aspirates and Mediæ, as well as those which actually exhibited Tenues, would, if our hypothesis were true, have been found with Tenues in the parent speech; that is to say, that that speech was always as copious in its vocabulary as all its component dialects were at the epoch

this fate a way of escape was found in the appropriation of the results of debilitation to the expression of new meanings; so that the very imperfections of man's constitution were, as they still are, pressed into the service of his intellect. Not that *all* such results were thus appropriated; far from it. So large an increase of *capacity* for expression (as I have called it) was more than was actually needed; for a considerable part of the primitive vocabulary, as well as of later ones, must have been of such a kind as not to admit of differentiation. To elucidate this statement, however, it may be desirable to devote a section or two to a somewhat fuller investigation of the mode of extending the powers of language by Phonetic Differentiation than was called for in section 17.

28.—(*a*) In that section, only a general outline of this principle is given; and in Appendix C an attempt will be made to confirm the accuracy of that outline by facts and opinions. The great truth involved in the principle is, that the distribution of meaning does not precede and govern, but follows and is subject to, phonetic variation. But although all such distribution is preceded by this variation, it is not true, conversely, that all such variation is followed by that distribu-

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just specified. This, again, would really mean that the demands on language were as multifarious in the infancy of mankind, when men had all their ideas to acquire, as in later ages, when men inherited the ideas which it took very many generations of their forefathers to accumulate. To this I demur; for whensoever and whencesoever man first appeared on the earth, it is certain that he then had both to acquire all his ideas and to construct or select for them some phonetic expression. Consequently there must have been a period in his history when the possible combinations of half a dozen different sounds would far exceed all his requirements. It was the accumulation and more precise distribution and subdivision of ideas that continually impelled him to burst through the narrow limits of expression which at first hemmed him in; and, accordingly, instead of supposing that the tracing of the two feebler mutes of each family to a single stronger one involves the permanent existence of numerous homonyms in the parent speech, I reversely suggest that the evolution of those mutes by debilitation offered the means of avoiding homonyms which must otherwise have come into being, and perhaps of getting rid of some that actually did for a time exist.

tion. Whether it is or not, depends partly on the intellectual requirements of the people, and partly on the nature of the several sections of the vocabulary operated upon. If a whole tribe of men of dull intellect remains content with words that express more or less than would be agreeable to clearer thinkers, or if the meaning of words cannot, or need not, be made more precise, then, although the phonetic embodiment of the meaning may change, the same single meaning will cling to all the changing forms. Phonetic variation therefore, viewed in relation to its linguistic uses, may be divided into two species—the *fertile* (say), and the *sterile*.

(*b*) Fertile Variation is that already described in § 17 (*d*). It occurs when, to a word whose original phonetic form gives off a second, third, or further dialectic form, there was originally attached a vague, wide, and general meaning, which similarly gives off a second, third, and so on, better-defined or specialized meaning—when, subsequently, these meanings become severally attached to each of those forms—and when the more definite words [1] thus generated are adopted by both or all of the commingled dialects. To take the simplest case: suppose there are two dialects, D and D', in contact with each other, of which D is the standard and D' the fluctuating dialect (§ 21); and suppose that in D', by the side of the original sound s of any word, there grows up a secondary sound s': then, if the original meaning m of the word be such as to give off a secondary meaning m', it may (not must) happen that sooner or later the two meanings will become severally assigned to the two sounds. There are, of course, four possible ways in which these two pairs of elements may be combined into words; viz.,

$$m+s,\ m'+s',\ m+{}'s,\ m+s';$$

that is to say, the old sound may cling to the old meaning, and the new sound and meaning may diverge in company (as represented by the first two of these formulæ), or (as represented by the second two) the old sound may gradually

[1] I use the term "word" to cover sound+meaning. It is often used of sound alone, as by Locke, generally, in the Third Book of his great Essay.

change its meaning, while the old meaning may gradually pair off with the new sound.

(*c*) The distribution of these combinations between the dialects will constitute two different systems, according to the mutual relationship subsisting between *D* and *D'*. If that relationship be very close and intimate, so that in point of fact the dialects are still one and the same, and unable to carry their differences beyond the origination of new sounds, then the same pair of words will finally be current in both; namely, either $m+s$ and $m'+s'$ or $m'+s$ and $m+s'$; but if, their cohesion being looser, they exhibit the partially independent relationship described in § 21, then the final distribution may be represented, for one dialect by the first two of these formulæ, and for the other by the last two. *D*, for example, will maintain the parent word $m+s$, and assign the new meaning m' to the new sound s': while *D'* gradually changes the sound attached to the old meaning; and when, through continued intermixture with *D*, it recovers (if indeed it ever lost) the older sound, it assigns thereto the new shade of meaning. Thus, finally, colligating like meanings, we have—

$$\left.\begin{array}{l} m+s \text{ in } D = m+s' \text{ in } D' \\ m'+s' \text{ in } D = m'+s \text{ in } D \end{array}\right\}; \quad . \quad . \quad . \quad (\xi)$$

or, neglecting the meanings,

$$\left.\begin{array}{lllll} D & \text{exhibits} & s & \text{and} & s' \\ D' & ,, & s' & \text{and} & s \end{array}\right\};$$

where the relationship between the two sounds is, *apparently*, Cross Compensation pure and simple (§ 18).

(*d*) Apparently, but not really; for some of the most important conditions (§ 22 (*c*)) of such Compensation are wanting. Thus, instead of two sounds (*s* and *s'*), there is, at the outset, but a single sound (*s*) common to both *D* and *D'*. This may seem a slight matter; but it is really a most important one; and correct views respecting it will, I think, elucidate more than one obscure linguistic phenomenon. For in Cross Compensation both *s* and *s'* are already native to *D* and *D'*; and the mere interchange of their places in the vocabulary

of D' does not in the least alter their phonetic value. But, in the case we are now discussing, s alone is native to both D and D': s' is a variation indigenous to D' only; and it may really be regarded as a foreign sound to D. To the section of people speaking D, therefore, s' may in fact offer some difficulty; and, in attempting to catch it up, being unable to reproduce it exactly, they may actually produce only some more or less close approximation to it. But further, as a consequence of the unity of the parent sound s, the phonetic change in each dialect is one of debilitation only, and falls completely under the Principle of Least Effort. D', therefore, is relieved, as it were, of the labour of raising a weaker sound to a stronger, in order to compensate for the descent of the stronger to the weaker: so that the whole process, when tested by the Principle of Least Effort, is less open to exception than the process of Cross Compensation itself.

29.—If now we suppose Fertile Variation to extend to three dialects, D, D', D'', of which D' (suppose) gives off a variety s', and D'' a variety s'', of the parent sound s, and in which the original meaning m gives off two related meanings m' and m''; then, supposing the dialects, as before, to be closely fused, there will be in them all an exact agreement in both sound and meaning; or the three resulting words,

$$m+s,\ m'+s',\ \text{and}\ m''+s'',$$

will (if required) become current in them all. But if the reciprocal relationship among them is similar to, and, indeed, merely an extension of, that between the *two* dialects, D and D', just now described, and tabulated in formula (ξ), then the verbal relationship may assume the following perfectly regular and symmetrical form:—

$$\left.\begin{array}{llll} m+s & \text{in } D & =m+s' & \text{in } D'=m+s'' \text{ in } D'' \\ m'+s' & ,, & =m'+s'' & ,, \quad =m'+s \quad ,, \\ m''+s'' & ,, & =m''+s & ,, \quad =m''+s' \quad ,, \end{array}\right\};\ \ldots\ (\pi)$$

or, the meanings being neglected,

$$\left.\begin{array}{llll} D & \text{exhibits } s, & s', & s'' \\ D' & ,, \quad s', & s'', & s \\ D'' & ,, \quad s'' & s, & s' \end{array}\right\}.$$

If the symbols **H**, **A**, **S** be substituted for s, s' s'', this tabulation becomes identical with that of Grimm's Law in § 2 (*e*). The case it represents is certainly a complex one; but, given the dialectic conditions, the case is one that *must* occur. The conditions are, it is true, extraordinary, but by no means impossible; for what is certainly true (as I shall presently show by an example) of two sounds in two dialects may be true of three sounds in three. Indeed everybody who takes in hand to account for the unique character of the phenomena summarized in Grimm's Law, expects to find that they result from extraordinary conditions; in proof whereof we have seen that those who have failed to discover, have sometimes not failed to invent, such conditions.

30.—Let us now pass on to *Sterile* Variation, so far at least as it is due to the nature of the section of the vocabulary operated upon. This would most naturally be exhibited in the case of words already denoting specific things and actions (*e. g.*, *patar*, *πατερ-*, *pater=vater=fadar=father*). In such instances the parent word, though gradually diverging in sound in different dialects, retains the same meaning in them all. If the dialects, after incipient divergence, again become completely fused, one form drives out the other or others (as, *e. g.*, the English *father* has served the twin-form *fader*): but, among dialects tending to division, each preserves its own special variety, whether it be the primitive or a degenerate form; while, as there are no new varieties of meaning to express, no dialect needs to adopt any varieties of form current in the commingled dialects. Hence the phonetic varieties are distributed individually through the individual dialects. That is, supposing D, D', D'', &c. to be the dialects, s, s', s'', &c. the corresponding varieties of the sound, and m the unvarying meaning, of a given word, then

$$\left.\begin{array}{lll} D & \text{will exhibit} & m+s \\ D' & ,, & m+s' \\ D'' & ,, & m+s'' \end{array}\right\}; \quad . \quad . \quad . \quad . \quad (\rho)$$

and so on, to any number of dialects and sounds. Or, as m

remains constant throughout, the variations in *D*, *D'*, &c. are confined to sound alone, and may be briefly represented by *s*, *s'*, *s''*, &c. If therefore for *D*, *D'*, *D''* we put our three leading I-E. dialects, then instead of *s*, *s'*, *s''*, we may read, as before, **H**, **A**, **S**,—the Cl., or L.G., or H.G. dialect being placed first (§ 2 (*e*)) according as one or the other maintained the parent sound. But as there was no special reason, outside phonetic action, why, in the case of Sterile Variation, any one dialect should catch up the forms generated in any other, I prefer to think that the fashion of symmetrical distribution was set by the action of Fertile Variation, on the plan described in §§ 28, 29, and that all sounds were ultimately drawn into the movement, and adjusted according to that fashion or pattern, by the principle of Analogy.

31.—(*a*) On the hypothesis, then, that **A** and were originally but different debilitations of **H**, we have found, I think, a theoretically satisfactory explanation of the origin and object of the great phonetic movement represented by Grimm's Law. Its origin is traced to the well-known cause of nearly all sound-change; its object was the extension and variation of the power of expression in accordance with an equally well-known procedure of language. The question arises, by the way, whether any facts in support of such an explanation are producible, not from modern languages, where they are innumerable, but from the Holethnic speech itself, so far as we are able to restore it. I think there are, and not a few; for among the primitive forms into which the common I-E. verbal stock has been resolved, there appear many groups or clusters of two or more roots, the members of which differ so slightly, both in form and meaning, that it is scarcely possible to avoid inferring their radiation from some single original centre. Many good examples of such radiation by simple *vowel*-weakening may be found in Fick's Wörterbuch[1]. What we are here concerned with, however, is radiation by *consonant*-change; and of this likewise, the traces, if less numerous, are not less distinct.

[1] Pp. 943–965.

(*b*) To quote an example or two: Prof. Max Müller reflects somewhat severely upon those "who think it mere pedantry to be restrained by Grimm's Law from identifying such words as *καλ-εῖν* and 'to *call*'—*corvus* and '*crow*';" for *call* and *crow* imply a (Cl.) primitive *gar-* (whence *γῆρ-υς*, *gar*-rulus, *gal*-lus, &c.), while *καλ-εῖν* and *corvus* imply a primitive *kar-* (whence *κορ-ώνη*, *κῆρ-υξ*, &c.)[1]. *Kar-* and *gar-*, therefore, must be kept distinct to a period beyond the Separation. But, *teste* Max Müller, what does *kar-* mean? "To shout, to praise, to record:" and what does *gar-* mean? "To sound, to praise;" *i. e.*, two roots almost identical in form are used to express the slightest possible varieties of meaning; and some of their remotest descendants still differ just as little (compare our "*to call*," which represents *gar-*, with "*to hail*," and "*to halloo*" from *kar-*). Now, what simpler explanation of such a remarkable twofold similarity can be conceived than that *gar* was originally a mere phonetic variation of *kar*, and that each was gradually appropriated to express one of a pair of scarcely divergent meanings? This is not all, however. There was certainly a third Cl. root of the same group,—viz. *khar-*[2] (*χαρ-*, ghar-), whence the Skt. reduplication *gharghara* "laughter" and the Gk. *χελ-ιδών*= Lat. *hir*-undo; to which answered the O.N. *gala* "sing"; A-S. *galan*; O.H.G. *kalan* or *galan*. The parent form, *kar-* (*s*) seems therefore to have given off two phonetic varieties, *khar* (*s'*) and *gar* (*s''*). These were caught up by the commingled Holethnic dialects and applied to designate varieties (*m'*, *m''*) of the one action of shouting or using the voice (*m*), which was more vaguely denoted by the parent form *kar*; so that from this group we can construct an almost complete exemplification of the symbolical scheme (*π*) in § 29:—

Cl. *kar-* (*καλ-εῖν*)[3] = L.G. *har-* (*hal*-loo) = H.G. [*hal*-ôn[4]]
„ *khar-* (*χελ-ιδών*) = „ *gar-* (*gal*-an) = „ *kar-* (*kal*-an)
„ *gar-* (*γῆρ-υς*) = „ *kar-* (*call*) = „ *khar-* (*chal*-lôn)

[1] Lectures, ii. 27, i. 416. [2] In defence of *kh* see § 33 *infrà*.

[3] *L* or *λ*, in the separate languages, generally represents a primitive *r*.

[4] By the hypothesis of §§ 52–54 *infrà*, this form in *h-* would represent an older form in *g-* (*gal*-ôn), which completes the scheme.

(*c*) Another example is furnished by the guttural demonstrative. The form of this root was originally perhaps *aka*[1], which, by varieties of accent, would give off the two forms *ak* and *ka*, or with vowel-weakening *ik* and *ki*, and others. Of all these the consonant is the characteristic part; so that we may put $s=k$, $s'=kh$, $s''=g$. In meaning the word originally covered all deiktic action irrespective of direction; but afterwards it and its varieties were appropriated to indicate three special directions,—that (yonder), this (by me), and this person (myself: *cf.* the Americanism "this child," or the negroism "dis nigger"). Thus—

Cl.	*a-ki* (*ἐκεῖ-νος*, Lat. *-ci*)	=	L.G.	*hi* (*he*)	=	H.G. (wanting)
,,	*gha* (*hi-c*, *ho-c*)	=	,,	*ga-*, *ge-*	=	,, *ka-*, *ki-*
,,	*agâ* (*ἐγώ*, &c.)	=	,,	*ik* (*I*)	=	,, *ich*[2].

(*d*) Perhaps not many cases can be produced in which a triform variety runs nearly through all the three dialects. This is hardly to be wondered at, seeing that every individual language inclines to certain favourite modes of word-formation and composition, the results of which ultimately drive out older and simpler forms. But any one who may wish to pursue the subject will find many examples of less regular differentiation in Holethnic times. Thus Fick[3] points out that nearly half the primitive (Cl.) roots ending in *-g* are by-forms to roots in *-k* of closely related meaning (*e.g.*, *arg-* "be bright" = *ark-* "blaze, gleam," nearly); and again, further on[4], that many in *-d* are similar by-forms of roots in *-t* (*e.g.*, *kad-* = *kat-* "fall")[5].

[1] See Appendix E.

[2] In tabulating the demonstratives we must bear in mind (1) that the guttural demonstrative threw off the relative series (*ka-*, *κϝο-* or *πο-*, *quo-* or *qui-* = L.G. *hva-*, *who*; as to the *w* of which, see *infrà* §§ 55–63); (2) that there were other demonstratives, which, especially those from the dental *ata* (*at* and *ta*), ultimately shared with the guttural the somewhat narrow area of meaning to be occupied.

[3] W-B., p. 982. [4] P. 1000.

[5] Fick, in his treatment of the "determinative" or final element *d*, appears somewhat inconsistent both with himself and still more with the probabilities of the case. After expending much labour to show that all the host of roots involving *i*, *u*, *â*, *î*, *û*, *ai*, and *au*, are derived by pho-

32.—(*a*) The second point proposed for examination in section 27 is the dialect or dialects of the Holethnos in which the phonetic movements leading to the evolution of the systems Σ_x, Σ_y, Σ_z may have arisen. In the prevalent theory of Grimm's Law this question is, of course, predetermined. One system (Σ_x) being assumed as the parent of the rest, its sounds (**H**, **A**, **S**) become an impassable barrier at which all inquiry necessarily stops. But if, at an indefinitely remote period, the Indo-Europeans, as I have tried to show, can have had no other Mutes than the Tenues, it is clear that the Mediæ and Aspiratæ must severally have made their appearance among them at a given time and in a given place. The time must be left out of the question. By the place we must understand (not a geographical, but) a dialectical position. Beginning with the Aspirate, I observe that the infection of Aspiration could not have broken out over all the parent language at once; for it would then be difficult, if not impossible, to explain why it did not affect the same part of the vocabulary throughout, but affected different parts in different dialects. Further, it would in that case have been

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netic variation from comparatively few roots involving *a*, and after suggesting no other origin for the determinant *g*, than that it arose from a weakening of *k*, he proceeds to argue against a similar suggestion in respect of *d* (viz., that it is a weakened *t*) chiefly on the ground of the rigid firmness of the Holethnic speech:—"Wir müssten dann für eine ferne Vorperiode unsrer Grundsprache ein Schwanken der Laute annehmen, wozu uns ihr sonstiger Granitbau gar nicht berechtigt. Alle stärkeren Consonantenwechsel und -wandel sind auf die Einzelsprachen beschränkt; erheben wir uns nur eine Stufe höher .... so hört schon fast alle Lautaffection auf....So also weiter zurückschliessend kommen wir zu dem Ergebniss dass die Ursprache völlig lautfest, keinerlei Wandel und Wechsel von harten in weiche oder gehauchte, von Gutturalen in Labiale, u. s. f., unterworfen gewesen sei" (W-B., p. 1000). From all this it would seem that a hard and fast line is to be drawn at a certain epoch, behind which language must be supposed to change no more; which is as reasonable and as probable as if a geologist should select one of his strata as the ultimate limit of cosmical change. The fallacy underlying such views is not far to seek; but it must here suffice to remark that if language ever presented the rigidity of form imagined by Fick, man must have been a very different being from what he now is.

native to all those dialects, and we should have expected to meet with a homogeneity of character and a uniformity of development which are quite at variance with the known nature and history of the sounds which represent it. Besides, there is the patent fact that certain tribes, whose Mute-system, in other respects, agrees with Σ_x, possess no Aspirates at all; which means, on our view, that they never did possess any. For it would surpass belief that those tribes should have taken part in a great aspirating movement, and yet that no trace of Aspiration should afterwards be discoverable among them.

(*b*) In suggesting an answer to the question, Where then *did* this phonetic infection originate? it is not without hesitation that I venture to differ from universal opinion on the point. Nevertheless I think the facts displayed in section 14 leave us no choice but to assign the parentage of Aspiration to the clan or division afterwards represented by the Low Germans[1]. I see no better way of explaining the overwhelming share which the spirants (*h, th, f*) take in the L.G. phonetic system than to refer it either to the great virulence of the original infection (indicating special affinity between the dialect and the sound),—or to the earlier period at which it set in, and consequently the greater length of time during which it operated,—or, still better, to both these causes put together. Subsidiary evidence to the same effect is deducible from the regular and early decline of all three Aspirates to Spirants in the purer L.G. dialects, such as the Gothic. There is a harmonious relationship between the latest forms of all the three which well accords with the supposition of a uniform process of debilitation operating on sounds whose original relationship was similarly harmonious[2].

[1] I shall continue to apply the abbreviated descriptions *Cl.*, *L.G.*, &c., to those divisions of the Holethnos which must have represented the ancestors of the later peoples so designated.

[2] An examination of the history of the three spirants, *h, th, f*, throughout the I-E. languages suggests that they are nowhere primitive sounds. Hence it is a weak point in the first-mentioned aspect of Prof. Max Müller's hypothesis (§ 23 *antè*) that he supposes the Germans to have had at their disposal—lying by them unemployed as it were—sets of Spirants which, according to him, could never have been Aspirates; but for whose

33.—(*a*) If, again, we take our stand upon the L.G. Aspirates as a centre, as it were, and look round upon the varieties of Aspiration furnished by the other I-E. tribes, the scene presented is one of complete phonetic bewilderment. The new sound to these other peoples was an apple of discord. The ancient Indians show us, in the main, a set of three *Soft* Aspirates (*gh, dh, bh*), which may all in some cases become the guttural breathing *h*; the ancient Greeks, a set of three *Hard* ones (*χ, θ, φ*), which in later times became Spirants; the Old Bactrians generally the three *Mediæ*, and some peculiar Spirants (*z', z, w*); the ancient Italians a couple of *Spirants* (*h* and *f*) as initials, and generally the three *Mediæ* within words; the High Germans Old and New exhibit an extensive variety of symbols (*ph, pf, f, v, z, tz, ss, ch, h*, and others), although the phonetic values of these may have been less various; while the Old Kelts and Lithu-Slaves exhibit no Aspirates at all, their place being taken by the Mediæ. Thus, to sum up:—The *Guttural* Aspirate, represented in L.G. by *h*, is represented in the other dialects by *kh, gh, χ, h, ch, f, g, z', z*. The *Dental* Aspirate, represented in L.G. by *th* (þ), is represented by *th, dh, θ, h, f, ss, ts* (*z*), *d*. The *Labial* Aspirate, represented in L.G. by *f*, is represented by *ph, bh, φ, pf, f, v, h, b, w*. Most of these incongruities are attributable to the different Cl. tribes; and yet, on the Chronological Hypothesis, it is among these tribes we are to look for the phonetic system which is to be taken as the standard and primitive system to which the others are to be adjusted.

(*b*) The explanation of all this phonetic diversity lies, I think, in the principle laid down in § 28 (*d*). A new sound, **A**, in each family of mutes, sprang up and spread widely in the L.G. dialect. The original phonetic value of the sound may not now be exactly recoverable; but whatever it was, it must have been strange and somewhat difficult to the surrounding

sudden appearance in their actual form he does not otherwise account:—"Aspirates, whether hard or soft, they [the Low Germans] had none; ... and in order to distinguish the third series [of mutes] both from the *g, d, b*'s and *k, t, p*'s, which they had used up, they had to employ the corresponding hard breaths *h, th*, and *f*."—Lectures, ii. 225, 226.

tribes; so that not only the wider Cl. and H.G. dialects, but their component subdialects, although otherwise in unison, were unable, when adopting the sound, to agree among themselves in giving to it its precise phonetic value, or indeed any uniform value at all; and only produced a set of more or less successful imitations, except where they gave up the attempt altogether, and took refuge in the Mediæ. That Cl. subdialect which seems to have approached more nearly than the rest to an exact reproduction of the Aspirate, if indeed it did not completely succeed, was the Hellenic. This is no more than might be expected from the fine ear which distinguished the later Greeks, and which was, no doubt, a characteristic of their early progenitors; and it is actually deducible partly from the history of the Greek Aspirates (which, like the L.G. Aspirates, have ended in Spirants); and partly from their nature as *Hard* Aspirates: for whoever will carefully consider the L.G. Spirants, both in themselves[1] and as reflected in their H.G. representatives, can scarcely come to any other conclusion than that the parent sounds must have been *hard* sounds. If then, the origin of the Aspirate is to be assigned to the Cl. tribe at all, the Hellenic subsection alone can claim to be its parent. This, however, would still be inconsistent with the small part which the Aspirates play in the Greek phonetic system, and would render it more difficult to explain why the other Cl. subsections should have differed so widely from the Hellenic. Those subsections moreover differed among themselves not only (apparently) in the original character of their Aspirates, but also in their modes of debilitation. These modes do not comport with the supposition that the Aspirates were indigenous; for while the purer forms of L.G. have preserved the gutturalism, dentalism, and labialism of the several Spirants down to the present moment, the Skt, for example, in cases where it debilitated the Aspirates, reduced them indiscriminately to the guttural Spirant *h*; and the Latin reduced the (initial) dental, and sometimes even the guttural, as well as the labial, Aspirate (or its imperfect representations of them) almost as indiscriminately to the labial Spirant *f*.

[1] See Appendices B and D.

34.—The doctrines of the last two sections, if accepted, will settle some very lively controversies,—settle them, however, by completely cutting away the hypotheses which give rise to them. Perhaps the most vehement of all is that which has raged about the questions, whether the Hard or the Soft Aspirate was the older, and which was the parent of the other. Kuhn[1], in reviewing Schleicher, maintains, with his usual sound judgment, that the Hard Aspirate was the older; and indeed this view is clearly the only one which is consistent with the Principle of Least Effort. Curtius, on the other hand, as we have seen (§ 12)[2], makes the Soft Aspirate the older, and derives the Hard from it. Once more, Grassmann[3] attributes both Hard and Soft Aspirates to the primitive speech,—rightly enough, on our hypothesis; but on the Chronological Hypothesis this still leaves the main difficulty unsolved—viz., why Hard Aspirates in Greek should almost universally answer to Soft Aspirates in Sanskrit. With one or other of these three leaders, but mostly with Curtius, nearly all subsequent writers on the subject have sided. For us, however, it is now needless to enter into the controversy. The various sounds of § 33 (*a*) not being derived one from another, and all being but so many dialectic reflections or attempted reproductions of a new and difficult sound which originated in a commingled dialect, their history and ultimate form differ in various directions from the history and ultimate form of the original sound. Minute phonetic twists in the several adopting dialects, imperceptible at the outset to the finest ear, might still wrench the sound on to widely divergent lines of debilitation; and hence the various *termini* (which alone we know) may be considered to be connected by those various lines, not actually with the genuine Aspirate, but with so many imitations standing in various degrees of proximity to it. The Aryan in both branches, and perhaps the H.G. and the Grk, erred, it would seem, on the side of too nearly complete a contact, although they may have differed one from another in point

[1] Z-S., xi. 308, &c.

[2] He discusses the question at length in his Grk Et., p. 423, &c.

[3] Kuhn's Z-S., xii. 81, &c.

of energy[1]. The Italians, and notably the Latins, seem to have possessed a very defective capacity for acquiring, or at any rate preserving, either the Aspirates themselves, or even their own substitutes for them. These people were perhaps of a dull ear and an unwieldy tongue. Their representations of the Aspirates accordingly exhibit not merely too great relaxation, but relaxation of diverse kinds—sometimes of contact and sometimes of energy. Not only did the initial Aspirates become Spirants, without exception; but these Spirants themselves, being indistinctly formed, mostly ran together into the one spirant *f*; while within words the less decisive efforts to acquire the Aspirate, or, rather, the influence of the vowels between which internal mutes generally fall[2], led to the production of the Media.

35.—(*a*) This phenomenon of the representation of the Cl. Aspirate by the Media, in other than the L.G. dialects, calls, perhaps, for a little further remark. The doctrine that the latter sound is the terminus to which the former, by natural debilitation, *ordinarily* tends, has already been controverted. It is not, even on the hypothesis of Curtius (§ 15), universally

[1] The Grk Aspirates could hardly at any time have been weaker than they were in Cl. times; but the Skt Aspirates may very well have been stronger. A general and uniform diminution of energy would not appear too violent in the latter language, in which whole families of consonants, unknown to the sister tongues, have been evolved out of the limited set which it once shared with these. But see *infrà*, § 45 *note*.

[2] The effects produced upon consonants by such bi-vocalism are in some languages very violent—a fact which is sufficient to render it altogether unsafe, when a consonant so circumstanced presents a different form from what is expected, to venture upon any decisive assertion respecting its history and its earlier forms. In the older stratum of French, as is well known, such medial consonants, even the strongest, have completely disappeared. In Greek we observe, on a smaller scale, a similar phenomenon in the case of internal σ and sometimes ν. The corresponding *s* of Latin did not indeed disappear; but it clearly indicates the influence of the conjoined vowels by its passage into *r*. The difference in the *termini* (*r* and nothing) reached by the Latin and Greek *s* respectively offers an illustration of the effect of that slight organic difference in the phonetic value of the same letter in two related languages which I have roughly described as a "minute phonetic twist" (See the first note to the next section).

true; and the suggestion (§ 25) that both **A** and **S** were directly and contemporaneously evolved from **H**, cuts away the main support both of that doctrine and of this hypothesis, which lay in the supposed passage of the **A** of Σ_x into the **S** of Σ_y. But the appearance of Mediæ for Aspiratæ in Latin, for example, still leaves the question undecided whether *genuine* Aspirates, which have certainly become Spirants on a large scale, could also descend to Mediæ by a divergent line of debilitation. For, according to the hints offered in § 33 above, the sounds representing the Aspirates in most, if not all, subsections of the Cl. tribe, would *not* be *genuine* Aspirates, but only the results of so many more or less successful efforts to acquire a new and strange sound: consequently, according to my views, the internal Mediæ of Latin would be evolved not from the genuine Aspirates, but from the approximative imitations of them, which alone the Italians, and especially the Latins, were able to produce, such evolution being modified by the potent influence of the conjoined vowels[1].

[1] The variations of phonetic value which a sound, generally considered one and the same, may really undergo, according as it is pronounced by natives of various districts and countries, and the effects of such variations upon the destiny of the sound, form a subject which has scarcely been sufficiently taken into account by writers on Vocal Phonetics or the Physiology of Speech. Yet these variations form an appreciable, nay a very important, factor in Phonology; to which, indeed, should be referred, for example, the various forms of attraction which a sound treated as one and the same exhibits for other different sounds in different languages, and ultimately the different history and fate of the same sound even in the related branches of one older language (See the previous note). The training of the vocal organs and the subsidiary muscular apparatus in different countries generally differs widely in a few points and slightly in a great many—as every one will admit who, whatever his nation, has listened to the early attempts of foreigners to speak his language, and has noticed the strangely different powers, the curious and inimitable twists, frequently given even to the simple vowels, and sometimes to consonants. The same is true to a less extent of dialects of the same language. The lisping Cockney, for example, can no more produce the fine brogues of our northern counties than he can the clicks of the Hottentot. Hence, if a language be imposed on, or adopted by, an alien race, some of its sounds will probably be at once wrenched from their native position; many more will undergo some less violent, and at first scarcely percep-

(*b*) But there may have been other modifying causes which ought to be taken into account. I believe too little weight has been given by philologists to the former reciprocal influence of commingled dialects. When two younger dialects in contact (D and D') exhibit related sounds (say *s* and *s'*), it is sometimes too readily assumed that one sound (say *s'*) is directly derived from the other (*s*), even when it is well known that D' (in which *s'* occurs) must once have been, for an indefinite period, in contact or commingled with some third dialect which also answers by *s'* to the *s* of D. Now, both the Li-Sl. and the oldest Keltic dialects, as regards their mute-system, belong in the main to the Cl. section; but that they ever possessed any Aspirates may be with absolute safety

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tible deflection; and both these and those may experience a totally different destiny from that which awaits them on their native soil. These are conditions which, owing perhaps to the prevalence of such views as are animadverted upon in the last note to § 31, have often been strangely overlooked, but which play an important part in the theory of this Treatise (See §§ 10 (*d*), 28 (*d*), 33, 34, 41 (*b*), 61, *et alibi*). [After this note was in type, there appeared in the "Educational Times" for Nov. 1, 1875, a Lecture "On the Acquisition of Languages," by perhaps the first English authority on Vocal Phonetics, A. J. Ellis, Esq., M.A., F.R.S., which in some parts offers such an apposite illustration of the foregoing remarks, that I have removed some additional ones I had made in order to find room for a quotation. Mr. Ellis is urging the importance of acquiring in childhood the correct pronunciation of foreign languages; and, in the case of German, he endeavours to bring out the very different phonetic value of letters which seem to the eye identical with English letters, by a minute analysis of a few simple phrases—*Brod und Milch*; *das Fleisch und die Sauce*; *etwas Branntwein und Wasser*—which really "bristle with sounds which few Englishmen appreciate, much less reproduce":—"In *Brod* the trilled *r*, the long *o* without any tendency to end in *oo*, the dental *d* (that is, with the tongue against the teeth, not against the palate, as in English), going off immediately into a dental *t*, so that *Brodt* was the older spelling. In *und* the *u* is our common *u* in *full*, but we do not have it before *n*, the long *n* going into dental *dt*, as before. In *Milch* the difference between *mil* and our *mill* in the vowel (very short *ee*) and dental *l*, and finally the palatal *ch* after *l*, which is not *sh*, nor a voiceless *y*, though something like both. In *das* the broad and very short *a*, not lengthened as in our *grass*. In *Fleisch* the very broad diphthong *ei*, almost our *aye*, the very hollow *sch*, hollower than our *sh*. In *Sauce*, the final *e*, more like our *a* in *idea*

denied. Instead of those sounds, both the Li-Sl. and the Keltic exhibit a series of Mediæ which, as I shall try to show (§ 46), originated independently. Such sections of the Holethnos as the Italian[1] therefore, among which Mediæ occur partially or irregularly instead of Aspirates, may very well have had their earliest and imperfect imitations of the new sounds warped, as it were, or determined by the corresponding pronunciation of other sections, such as the Keltic or Li-Sl., which were still in contact with them[2]. Precisely the same thing occurs, especially as regards the Dental Aspirate, in many of the L.G. dialects, where *d* occupies the place which in the

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than anything else, and the peculiar pronunciation of the initial *s* as a slight *s*, followed by a full *z*. In *etwas* the broader *e*, dental *t*, lip *w* (a *v* without touching the teeth, not our *w*, nor *v*, nor *vw*), the distinct unaccented syllable *was*. In *Branntwein* the broad *a* and *ei*, and the same *w* as before, the long *n* before *t*, the dental *t*, really *dt* as before, and the final long *n*. In *Wasser* the *w* and *a* as before, the final trilled *r*. It is a very long while, indeed, before English organs can form these sounds readily and without effort. The English *w* is so especially difficult to Germans that I have seldom or ever heard one, even after years of practice, who could produce it. The German *w* offers the same difficulties to Englishmen."—As if by way of praxis on these remarks, the public were favoured by "Punch," a week or two later, with a sketch of a German tenor warbling "The Last Rose of Summer"; and this is how he is represented as "transplanting" one of the stanzas:—

> "I'll not leaf zee, sow lône von, To bine on ze schtem!
> Zins ze lôfly are szchleeebingk, Côh! szchleeeb sow fiz dem!
> Zos ghyntly I schgadder Zy leafs on ze bet,
> Vair zy maids of ze kârrten Lie schentless and tet!"]

[1] See note 1 to § 32.

[2] The solitary case of *mihi*, although insufficient for the basis of an argument, because it *is* solitary, is nevertheless of some interest as indicating that an attempt really *was* made to pronounce an internal Aspirate. In this particular case it was probably (as Curtius, I think, somewhere suggests) a species of dissimilation, *i. e.* an effort to avoid the close proximity of two labials (*mibi*) which operated to prevent the formation of *b*. Probably, too, the effort in this case likewise was assisted by the influence of another closely related dialect, viz., the Skt., where we find *mahja* (for *mabhja*). On the theory of the text, however, *mihi* proves nothing in reference to the Mediæ which represent internal Aspirates in other Latin words.

purer forms of L.G. is occupied by *th* (þ, ð) [1]. And thus, as the Italians consorted with the Keltic or Li-Sl., so various L.G. subdialects which were unsuccessful in catching up the correct sound of the dental Aspirate, consorted with the closely commingled H.G. dialect [2]. In short, if we base our idea of the connexion and relationship of the dialects and subdialects of the Holethnos upon our observation of the dialects and subdialects of living languages (and surely this is our safest method), we shall not imagine them to have constituted two or three great dialects perfectly uniform and homogeneous within themselves, but broadly marked off from one another; much less shall we dream of the Holethnic speech as of one rigid and granitic whole, whose constituent elements were exempt from all change either relative or absolute: we shall rather judge that at some few centres the characteristic dialectic differentiæ were developed to the utmost point consistent with general linguistic unity, while intervening dialectic areas exhibited every possible variety and combination of agreement and difference with each and all of the principal centres.

36. The same line of argument which assigns the origin of **A** to the L.G. dialect, would assign the origin of **S** to the H.G. dialect. Its range, both in Σ_x and Σ_y is too limited to justify us in placing its origin in either the Cl. or the L.G. dialect, whereas in Σ_z its range corresponds (or probably once corresponded) to that of **A** in Σ_y and to that of **H** in Σ_x. Besides, the main characteristic of Σ_x was (as we shall presently see) *Resistance* to infection; and as to Σ_y, the ravages of its own single infection were so extensive that to superimpose a second thereupon would be to represent it as most exorbitantly and even incredibly corrupt. Being an easy sound, the Media offered no opportunity for that diversity of pronunciation which presents itself in the case of the Aspirate. It might be proposed, however, as a question for speculation, whether **S** thus originated in H.G. quite independently of **A**, or whether **A** may

[1] See Appendix D.

[2] But in this case the possibility of subsequent assimilation must be borne in mind (See §§ 52–54, *infrà*).

not first have made some little progress among the L.G. tribe, and **S** have arisen from the earliest crude efforts of the commingled H.G. section to imitate that sound,—their efforts, like those of the Latins (§§ 34, 35), being diverted by extraneous causes to a different line of phonetic development. In this case the two dialects would have been previously in intimate combination, their tendency to loosen out into two would have been quite latent, and this new phonetic diversity would simply have made the tendency overt. The dissimilating effect of the correspondence between **S** in one dialect and **A** in the other, would still, of course, be precisely the same as if **S** had originated independently (§ 41 (*b*)); and the sentiment created would be strong enough to excite, at a later period, a more successful attempt to adopt the Aspirate. This view is supported chiefly by the fact that the H.G. medial affection attacked the same section of the original series of Tenues as did the L.G. aspirate affection; but it appears at first sight inconsistent with the probability that **A** and **S** (as will presently be shown) began to infect the principal Cl. dialects almost or quite simultaneously. Still this does not necessarily prove that they *originated* at precisely the same moment; for dialectic peculiarities may exist and even spread in their native dialect for a long time before they acquire the power to affect a neighbouring one.

37.—(*a*) The third point proposed for examination in § 27 is the symmetrical distribution of the related Mutes among the principal tribes of the Holethnos—or, what is the same thing, the cause of the functional relationship existing between the principal Indo-European phonetic systems. This is to be looked for in the dialectic condition, and in the relationship of the dialects, of the Holethnos. Upon the Chronological Hypothesis that condition, up to the very moment of the original fissure, may be described as a homogeneous unity, implying a corresponding unity and close cohesion of the people up to the same epoch. Our hypothesis, on the other hand, requires that the dialects of the Holethnos should have already displayed the expansiveness and variation consonant with the

character of a primitive language spread over a wide area, and should in fact have reached the stage befitting a people ripe for the division and subdivision to which they actually became subject[1]. In the latter condition, the rival dialects of a language assume, or tend to assume, coordinate importance: and if no external agencies operate to check their expansion and divergence, they ultimately become distinct languages. Such counteracting agencies appear, historically, in various causes of social and political compression, which reconsolidate the diverging sections of people, give to one dialect the paramount importance of a standard or polite dialect, and at last bury the rest in obscurity. Every great language of a great people offers an example of this process.

(*b*) But what we are here concerned with is the opposite process of ethnic and linguistic repulsion, in virtue of which divergent sections of a nation or tribe tend to become distinct peoples, and their dialects to become distinct languages. In watching this process we observe two linguistic stages which are (or may be) marked by different characteristics: the first extends over the time that the tribes remain in presence of one another; the second begins from the epoch at which they part company. In the latter stage (which we will first dismiss, as not much concerning us here), the dialects necessarily cease to have any direct mutual or reciprocal influence. Each now follows its own line of development as an independent language; and this (phonetically) consists in the modification and sometimes complete rejection of sounds which, under new

[1] "Das spätere Indische, Eranische, Griechische, Italische, Keltische, Lituslavische, Deutsche, so wie die übrigen für uns verschwundenen Sprachzweige, waren bereits auf engem Raume leise von einander gesonderte *Mundarten*, die noch stets mit einander in enger Berührung standen und vielfach gemeinsame Schicksale hatten. Nicht durch Auswanderung in weite Fernen haben sich diese Sprachen gesondert, sondern *durch Dialektbildung haben sich diese Volkstämme einander entfremdet* und erst *in Folge dieser Entfremdung* haben sie ihre weiteren Wanderungen angetreten" (E. Förstemann: Gesch., i. 241). These sensible remarks, however, are offered merely as an alternative hypothesis in explanation of the relationship of the Li-Sl. to the German on the one hand and to the Eranian on the other.

conditions, it finds troublesome to produce. Even in this case the operation of common physical and mental characteristics occasionally leads to similar or identical results in the different related languages; but those results are not correlated in a manner implying systematic *reciprocal* action. In the former stage, however, it may be otherwise; for commingled dialects, as we have seen (§§ 18–22), and as we shall again see, have the power of exciting in one another reactionary and counterbalancing changes; so that, when each at length becomes an independent language, it may still show deep-scored marks of its long conflict and ultimate instinctive compromise with the others. Hence, conversely, wherever, among a number of related languages, we discover reciprocal, symmetrical, or compensatory phonetic relationships on any considerable scale, we may conclude that the movements leading thereto must have taken place when those languages existed as dialects in presence of one another. And thus, in the case of the related systems Σ_x, Σ_y, Σ_z, it may be asserted on this ground alone, apart from any other objection, that the doctrine of independent and successive evolution lies quite outside the bounds of probability.

38.—(*a*) But in order to conceive with clearness the *rationale* of the symmetrical distribution of the mutes exhibited by those three systems, we must revert to our inquiry into the process of Cross Compensation (§§ 18–22), and to the modification of its results subsequently arrived at in sections 28–30. The former proves language to act in the very manner, but with even greater vigour than, we require; the latter gives the effect which would be produced upon phonetic distribution if certain conditions of the first-named process were modified. With all this in mind, I propose to resume, from § 18, the examination of the interchange between *v* and *w* in so-called "vulgar" English, as compared with the standard and "polite" dialect. To understand its origin, we must recollect the violent assaults to which our language has been subjected, and the composite nature of the English Vocabulary which is a result thereof. A glance at

any English dictionary shows that nearly all our words beginning with *v* have come to us from or through the French as the result of the Norman Conquest; while all those beginning with *w* are of native origin, and therefore, for us, of far greater antiquity. We are thus carried back to a time when in the popular language the sound represented by *w* was the only labial semivowel. For a long time after the Conquest, as is well known, the ruling class and the subject class spoke distinct languages. But social and political conditions made it inevitable that both an ethnic and a linguistic fusion should take place. The two languages, therefore, at first gradually, and at last rapidly, approximated to each other, until they assumed the form of two commingled dialects.

(*b*) Now observe what must have taken place over a certain area with respect to *v* and *w*. The standard dialect, while maintaining the native sound in its place, adopted also the related sound *v* from and with the language of the court and nobility. The immediate effect of the appearance of the new sound in one stratum of language was to evoke instinctive resistance in the close-lying popular stratum. And this resistance was not content to manifest itself passively by a mere retention of the native *w* and refusal of the foreign *v*; it further proceeded actively to turn (probably one by one, as it acquired them) all the intruding *v*-sounds into *w*'s. But as the two dialects continued in presence of, and active communication with, each other, this transmutation of *v* to *w* generated (§ 22) a sense of incongruity in the popular ear, which incited to an effort to correct it,—and at the same time a Dissimilating Sentiment, which diverted that effort to the production of a counterbalancing incongruity, in the change of the native *w* into *v*: and thus phonetic resistance gradually passed into a new stage whose results appear to be in direct contradiction thereto, and which may be designated phonetic retreat. It might certainly have been expected that a dialect which had evinced such phonetic vigour as to transform a foreign to a native sound, would endeavour still more obstinately to protect its native sound from change. But it did not: the gra-

tification of the Dissimilating Sentiment overrode even so apparently natural a tendency.

39.—(*a*) Examining this example more minutely, we observe that the polite or standard dialect distributed the two sounds, *w* and *v*, to their proper places in the vocabulary with almost complete exactitude, while the lower dialect responded by an order of distribution precisely the reverse. Hence in the case of the latter we are presented with the curious phenomenon of a cross-transfer of a foreign sound to native words and of a native sound to foreign words. Thus we have[1] (initial) *V*un (one), *V*unce, *V*ensday, *V*y (why), &c.; but *W*oters, *W*illin (Villain), *W*agabone (Vagabond), &c; and (within words) Pick*v*ick, Arter*v*ards, Camber*v*el, &c.; but Penton*w*il, Aggera*w*ating, Ad*w*antage, Di*w*ulge, Incon*w*enience, Sur*v*ive, In*v*ent, Per*v*erse, &c. This interchange appears most striking in the case of pairs of words derived from the same ultimate roots; as

*V*orm	for	*W*orm,	but	*W*armint	for *V*ermin;
*V*ord	,,	*W*ord,	,,	*W*erb	,, *V*erb;
*V*ine	,,	*W*ine,	,,	*W*inegar	,, *V*inegar;
*V*ay	,,	*W*ay,	,,	*W*oyage	,, *V*oyage;
*V*ant	,,	*W*ant,	,,	*W*ain	,, *V*ain;
*V*aggin	,,	*W*agon,	,,	*W*ehicle	,, *V*ehicle; &c.

(*b*) The infection of the native *w* at last became so virulent as to attack it in cases where we might have expected it to be protected by a neighbouring consonant; as, *sv*ig, *sv*indle, per*sv*asion, *sv*allow, *tw*enty, &c. It even operated upon the *sound* of *w* irrespective of the symbol and of the origin of the words in which the sound occurred; *e. g.*, rek*v*ire, rek*v*est, *kv*ite, *kv*estion, for re*qu*ire, &c.; and so vigorous were its assaults, that where *u* is the usual diphthongal *u* (*yoo* = French *iou*) it

[1] Nearly all the examples in the text are "Sam. Veller-isms" taken from "Pickwick." The following is from an amusing Introduction (by "Jacobus Baily, M.A.") prefixed to some old editions of Dalzel's "Analecta Minora":—"Literas *W* et *V* . . . affinitate inter se conjungi nemo hodie opinor denegabit. Rem dudum ad liquidum perduxerunt Cockneyenses, . . . 'Weal, *V*ine, and *W*inegar are *W*ery good *W*ictuals I *W*ow!'"

first resolved the diphthong into its constituent vowels, *y* (=ĭ) and *oo* (=*w*), and, while preserving the former intact, transmuted the latter into *v*: *e. g.*, [Sam*u*el, Sam*iw*el] Samivel; [sit*u*ation, sit*iw*ation], sitivation; [grad*u*al grad*iw*al], gradival; [punct*u*al punct*iw*al] punct*iv*al; [Jan*u*ary Jan*iw*ary] Janivary, &c. Some of the intermediate forms, however, were used almost as often as the final ones; and a few (*e. g.*, sit*iw*ation, act*iw*ally, Jani*w*ary) seem in some quarters to have been completely arrested *in transitu*.

(*c*) No doubt there were many exceptions to this substitution even during the time it was in most vigorous action; and if any competent observer had investigated them at that period, they might probably have been satisfactorily reduced to some rule; *e. g.*, the pronoun *we* was seldom if ever altered, perhaps because its continual employment by both dialects had led to their assimilation in this point. So, "werry *w*ell," not "werry vell," the second *w* being perhaps preserved by attraction of the first. Again, internal *v*'s in monosyllables were never changed (mo*v*e, sa*v*e, &c.), nor yet the second of two successive *v*'s (con*v*i*v*ial, sur*v*i*v*e, &c.); for a change in such words would have completely destroyed their character, and prevented their recognition by the commingled sections of people.

(*d*) With these exceptions, however, we are not greatly concerned; our business is with the normal process of interchange. It is clear that the maintenance of this is dependent on the mutual relationship between the two dialects affected; and it becomes interesting to conjecture what must have happened upon given perturbations of that relationship at successive periods in the history of the phonetic movement. If the two dialects had been completely separated after the assimilation of *v* to *w* had commenced, and before the dissimilation of *w* from *v* had set in, we should subsequently have found in the one (the upper or polite) a series of *v*'s and a series of *w*'s; in the other (the lower and popular) a series of *w*'s only, which would have had to do duty both for the *w*'s of the first dialect and for so many of its *v*'s as had been adopted and transmuted by the lower dialect before its separation from the upper. Again, if such separation had taken place

at the instant when Dissimilation had produced its most extensive effect, then we should afterwards have found two languages coexisting, to nearly every *v* in one of which there would have permanently corresponded a *w* in the other, and to nearly every *w* a *v*. But if we suppose the two dialects to remain in presence of each other, then precisely the same correlative interchange of sounds would continue regularly between them, provided that, and so long as, they mutually maintained the same imperfect degree of linguistic cohesion. In point of fact we know that, as long as the last-named condition holds good, Dissimilation remains, so to say, a permanent vital process, not executing certain effects once for all and then expiring, but continuing to seize on and transmute all the suitable sounds which, like many of the examples just quoted, may be introduced at successive periods into the dialect. But circumstances totally independent of language have, in the case we are studying, made it impossible for such a finely-balanced dialectic relationship to be permanently maintained. Semi-attached, and even hostile classes of society are being welded into a nearly homogeneous nation. A single dialect is driving its sister dialects out of the field; and all the conditions of social and political life accompanying our advanced civilization are altogether adverse to rebellion against polite diction. In a word, the schoolmaster is abroad, and the most obstinate provincialisms and vulgarisms are doomed to extinction.

40.—(*a*) Here, then, is a genuine and unmistakable case, apparently of Cross Compensation, but really of Dissimilation, growing up, flourishing, and decaying under our very eyes. It is, however, the simplest possible case, involving only two sounds in each of two dialects, and in such a way that, to every *w* in the one there corresponds a *v* in the other, and to every *v* in the one, a *w* in the other, in exact conformity with the formula of § 28. And just as this formula is mathematically a stepping-stone to the next more complex one (§ 29) involving three dialects and three sounds, so the simple example we have examined furnishes, out of the facts of language, a clue to the understanding of the next more complex example ex-

pressed by Grimm's Law, the difference between them being a difference of scale only. It is not quite impossible that somewhere among the obscure dialects of the world an equally complex example may yet come under observation; till then, Grimm's Law must be regarded as resulting from a unique combination of conditions which may, indeed, astonish us by its strangeness, but a comprehension of which now lies within our power.

(*b*) In reasoning from the simpler case to the more complex one, it is only necessary to see that the additional essential conditions and phenomena differ, not in kind, but merely in number—and that the subsidiary ones, if there be any, do not affect the *nature* of the others, but, like them, fall under known linguistic principles. Now, the essential conditions and phenomena of the simplest case, as deducible from the example just considered, are these:—(1) There is originally a single language employing a single sound of a certain character (*w*); (2) this language divides, or tends to divide, into two dialects; (3) into one of these dialects a closely related sound (*v*) is introduced; (4) the other dialect at first refuses to admit the new sound, and, on acquiring words in which it occurs, assimilates it to the native sound (*w*); (5) the two dialects continue in presence of each other; hence (6), by the growing habit of turning *v*'s into *w*'s, the perception of incongruity and the Dissimilating Sentiment are at last awakened; and (7) under the influence of the former, men half-consciously proceed to adjust their sounds to those of the polite dialect, but, diverted by the latter, their efforts only result in a counterbalancing transmutation of their native *w*'s into *v*'s.

41.—(*a*) This process is an exact practical exemplification of the theoretical table (ξ) of § 28. What, as was there conjectured, *might* take place, that, as is here shown, *has* taken place. In all essential points the actual and the ideal case agree. But what cannot be too often or too forcibly insisted on is, that the primary condition of these linguistic phenomena is one and the same, viz. the contact or commixture of the dialects in which they occur during the period of their evo-

lution. It is in virtue of this that the phonetic movement in one dialect evokes resistance, dissimilation, and counter-dissimilation in the other; and the absence of this condition (that is to say, the existence of the opposite condition of independent development) explains why, in cases like that of the Romance languages (§ 10 (*g*)), no such functional relationship can be traced and established, however closely in their radiation from a common centre, or in other respects, they may resemble the older I-E. languages.

(*b*) The conditions of this form of Differentiation which I have alluded to as non-essential, or subordinate and variable, are such as these:—(1) The manner in which the new sound makes its appearance. It is indifferent, for example, whether that sound is really a foreign sound introduced directly from abroad into one of the dialects (as in our example), or whether it is an indigenous phonetic variation in one dialect of an old sound common to both: in the latter case, it is, just as much as in the former, a foreign sound to the commingled and resisting dialect. (2) The dialect in which the new sound makes its appearance. This may be (as in our example) the one which in other respects is the standard or polite one, and which exhibits that character by maintaining the native sound in its original place, and merely adopting the new sound in conjunction with a forced addition to the resources of its vocabulary: it is in the popular dialect, as usual, that the greatest fluctuation exhibits itself. (3) But even this difference of the dialects in point of stability is non-essential. A standard dialect is in some sense an artificial product of extraneous circumstances, social, political, and (in civilized times) literary and educational: but in rude and primitive times one dialect may be as good as another; so that when such phonetic movements as those we have been considering set in, instead of one dialect remaining comparatively stable and fixed, and the rest differentiating their sounds against that one, all the dialects involved may be in every respect equal and coordinate; each may stand firm in some point where the other or others vary, and each may vary in some point where the other or others stand firm. Thus the popular transformation of *v* into *w* may have been

at first assisted by a corresponding habit of the Normans of turning *w* into *v*. (4) The number of new sounds which may severally be introduced, and (5) the number of dialects into which the original language may divide or tend to divide, are not limited by any inherent necessity. Neither (6) are the circumstances effecting or tending to effect such division necessarily uniform; so that, instead of finding the cause in a great political and social movement as in § 38, we may find it in the incipient separation of a large tribe of people into smaller ones, as the result of simple numerical increase and continuous extension over a wider and wider area. Once more: (7) In the example we have examined, the two sounds (*v* and *w*) of the one dialect are exact equivalents of those in the other. This is an accident, due to the feebleness and easiness of the sounds. In Cross Compensation such equivalence is essential (§ 22), because both sounds already exist in both the dialects involved, and merely exchange places, and not their phonetic values, in one of those dialects. But, as has been shown (§§ 28, 33, *et alibi*), when two dialects originally possess a single sound of a certain character, and a new variety of the sound enters one of them, it does not necessarily follow that the other dialect should exhibit an exact reproduction of that variety.

42.—(*a*) As the process described in the last few sections differs in two or three important points from Cross Compensation, it is desirable to find for it some special and appropriate designation. I propose therefore to call it "Reflex Dissimilation" or "Induced Phonetic Variation"[1], or, when

[1] The epithet "Reflex" is borrowed from the language of Physiology; which employs it when irritation of an "afferent" nerve causes respondent action (often involuntary and unnoticed) in a muscle supplied by the conjoined nerve of motion. The epithet "Induced" belongs to Electromagnetism; where it is used to denote the current excited in one conducting medium by the passage of a current through a medium in the immediate neighbourhood. In justification of the latter analogy compare the following definitions with the linguistic processes described in the text:—"Un courant qui commence fait naître dans un circuit voisin un courant de sens contraire"; "Un courant qui finit fait naître dans un circuit voisin un courant de même sens". ("Cours Elémentaire de Physique, par A. Boutan et J. Ch. D'Almeida": Paris, 1874; vol. ii., p. 148.)

two dialects are involved, "Mutual Differentiation", or, when more than two, "Reciprocal Differentiation". And as we have turned aside to define, it may here be convenient to collect and discriminate all the most important terms which have now been introduced in different parts of this Treatise.

(*b*) Phonetic *Resistance* implies that one dialect maintains an older, stronger, or purer sound against some variation or variations of it which make their appearance in some commingled dialect or dialects. If each of two dialects maintains such a sound in cases where the other debilitates it, the Resistance is *Mutual*. If such Resistance extends to three or more dialects, it becomes *Reciprocal*.

(*c*) The opposite of Resistance is *Retreat*; and this term, or some synonym, may be used to designate the correlative behaviour of a second dialect in relation to a resisting dialect, —*i. e.*, its debilitation of a sound in those cases in which an older and firmer sound is maintained by the latter.

(*d*) By the *Dissimilating Sentiment* is meant (See note to § 22 (*c*)) an impression generated in the popular mind either by Resistance or by Retreat on the part of a commingled dialect; which impression, growing out of one established phonetic difference, instigates to the involuntary execution of a second and counterbalancing difference.

(*e*) If these differences affect two preexisting sounds, and if all the Resistance is on the part of one dialect, and all the phonetic movement takes place in the other, then the result in the latter dialect is *Cross Compensation*.

(*f*) But if each of two or more dialects which originally had a single sound in common, while resisting where the other or others retreat (*i. e.*, debilitate that sound), itself retreats where the other or others resist, then we meet with the varieties of *Dissimilation* defined in (*a*) above.

43.—(*a*) The preliminary investigations now concluded have furnished, I think, all the means required for explaining the more complex system of phonetic distribution represented by Grimm's Law; for the difference in point of *fact* between this system and the simpler (dual) system examined in

§§ 38–41 corresponds precisely to the difference in point of *theory* between the formula (π) of § 29 and the simpler formula (ξ) of § 28. The conditions necessary to produce results expressible by the simpler formula have been shown to have really existed and to have produced such results. The conditions necessary to produce results expressible by the more complex formula are so rare that perhaps they cannot now be observed at all. But as to the existence of the results themselves there can be no mistake; and from these the existence, at however remote a period, of the conditions essential to produce them is an obvious inference. To connect the one with the other, I will endeavour to trace out in detail, upon a plan similar to that adopted in § 40, the phonetic movements which, upon the hypothesis of this book, should have taken place.

(*b*) In the simpler case we had to deal with a single sound, originally common to *two* dialects, into *one* of which a new variety of that sound was introduced. In the case now before us we again have to deal with a single primitive sound of each family (the Tenuis, **H**); but it was originally common to *three* commingled dialects, in *two* of which *two* new phonetic varieties (**A** and **S**) sprang up,—one sound in each. Bearing in mind this necessary extension, let us try to follow point by point the probable course of the more complex phonetic movement, as indicated by the simpler one detailed in § 40:— (1) There is originally a single language (the Holethnic) employing a single set of sounds of a certain character (the Tenues); (2) this language divides or tends to divide into (for our present purpose) *three* dialects; (3) in one of these three dialects (the L.G.) there springs up a new and closely related set of sounds (the Aspirates), and in a second dialect (the H.G.) there springs up, about the same time, a second new and closely related set (the Mediæ), both sets derived from the original series of Tenues by debilitation native to the several dialects, and both affecting the same section of that series; (4) the third (Cl.) dialect offers Resistance to each set of these derived sounds by maintaining the primitive Tenues in those places in which the other dialects had introduced each its own form of debilitation; (5) all the three in-

cipient dialects, with their newly established points of difference, continue in presence of one another; and hence (6) a perception of incongruity and a Dissimilating Sentiment are awakened in the corresponding sections of people; (7) each section therefore proceeds to execute Reflex Dissimilation against each of the others as completely as is possible under the complex conditions of the case. (And here, again, the two German dialects were probably the first to move. **S** in H.G. already corresponded to **A** in L.G.; and it remained for each of these two dialects both to adopt and to refuse the new sound of the other on the principle of Mutual Resistance and Retreat; that is, the common remainder of the primitive Tenues had to be distributed between **H** and **S** in one dialect (L.G.) and between **H** and **A** in the other (H.G.), so as to avoid coincidence of sound, and to exhibit the nearest approach to complete Dissimilation. This, it is clear, could only be done by making **S** and **H** in L.G. correspond to **H** and **A**[1] in H.G., or *vice versâ*.) (8) In the Cl. dialect a *dual* Dissimilating Sentiment was created by the twofold Resistance mentioned in (4): hence, when it proceeded to gratify that sentiment by modifying its primitive Tenues, it could only do so *perfectly* by retreating where each of the other dialects maintained the Tenuis, and retreating in such a way as not to agree with or accompany either; and this it did by adopting the Media where Σ_y maintained the Tenuis, and the Aspirate where Σ_ϵ maintained the Tenuis. To all this history may be added (9) that the fate of this complex Dissimilation differs from that of the simpler one examined in § 39. The latter is in process of extinction, if not already extinct, by gradual reassimilation of the vulgar to the standard dialect. But in the former we see that the phonetic equipoise ultimately established was maintained until the incipient grew into a complete separation of the dialects. Thenceforth, as independent languages, they severally preserved in a fixed and, as it were, fossilized form the several phonetic systems which had been previously

[1] **H**, **A**, and **S** are here employed to denote the whole series of Tenues, &c., in each dialect.

moulded by long-continued reciprocal action and reaction among themselves.

44.—*Remarks* on the foregoing section.

(*a*) No special phonetic affection has been attributed to the Cl. dialect; for as Σ_y and Σ_z are the systems of greatest debilitation, each in its own direction, so Σ_x is the system of greatest resistance. In its subdialects, out of all the mutes no less than two thirds (§ 14 (*c*)), on the average, are Tenues. Hence there may after all be some justice in treating the Cl. system as superior to the other two, not because it was anterior in time, but on the ground of its least deviation from the earlier phonetic condition in which *all* the mutes were Tenues.

(*b*) As the relationship between Σ_x, Σ_y, and Σ_z is perfectly symmetrical, it is really indifferent as regards the ultimate distribution of the sounds, which system be assumed to move first. If the Dissimilating process I. have endeavoured to establish be accepted as a genuine linguistic principle, its results will assume the same form, even though some one should hereafter contend that **A** or **S**, and not **H**, was the stronger parent sound, and that the new varieties thereof arose in the dialects taken in a different order from that hereinbefore adopted.

(*c*) The initial correspondence of **A** in L.G. with **S** in H.G. being granted, there was but one other way, besides that stated in § 43 (*b*, 7), in which each of these dialects could adopt the sound of the other while still preserving a section of the Tenues; namely, by making **S** in L.G. correspond to **A** in H.G., giving

$$\left.\begin{array}{l}\Sigma_y = \mathbf{A}, \mathbf{S}, \mathbf{H}, \text{ or } \mathbf{A}, \mathbf{H}, \mathbf{S}; \\ \Sigma_z = \mathbf{S}, \mathbf{A}, \mathbf{H}, \text{ or } \mathbf{S}, \mathbf{H}, \mathbf{A}.\end{array}\right\}$$

These results, as compared with perfect Dissimilation, are defective in two ways: (1) the relation between **A** and **S** resembles that simple Dissimilation (§ 28) which would here imply the introduction of *one* new variety of one older sound *other* than **H**, whereas *two* such varieties of **H** itself are introduced; and (2) a section of one system (the Tenues) remains iden-

tical with a section of the other, so that Dissimilation is to that extent incomplete. Such results as the latter, however, could hardly occur, because it is the Resistance of the older and stronger sound in one dialect which leads to Retreat in the other (§§ 22, 40, 42); so that, whatever sounds might agree in the different systems, the Tenues should not—as, in fact, they *do* not.

(*d*) Similarly, if Σ_y and Σ_z be supposed to have assumed their actual order and relationship, and the Cl. dialect to have admitted (as it did) both of their debilitations, the arrangement of Σ_x (**H**, **A**, **S**) could not be perturbed—that is, the debilitations could not have exchanged the sections of mutes which they affected—without producing agreement, in one section with one of the first-mentioned systems, and in another section with the other.

(*e*) For distinctness of treatment the action of the Cl. dialect has been represented in § 43 (*b*, 7, 8) as *subsequent* to the completed movements in the two German dialects. The latter movements ought certainly to have taken the direction they did, independently of the action of a third dialect upon them; but it is more probable that, almost from their commencement, they were assisted by a respondent movement in the Cl. dialect. A very few instances of symmetrical correspondence running through all three dialects, especially if occurring in words of every-day use, like the pronouns (§ 31(*c*)), would establish a precedent under which all later sound-changes would tend by analogy to range themselves.

(*f*) One and the same symbol **A** has been used to denote the Aspirates of all the dialects; but (§§ 28, 33, 41 *ad fin.*) its real value varies slightly as we pass from one dialect to another. If **A**, representing the original genuine Aspirate, be assigned to Σ_y alone, some other but similar symbols, say (α) and (ɑ) might be used to designate respectively the Cl. and H.G. varieties of the Aspirate; so that our expression of Grimm's Law becomes

Σ_x = **H** (α) **S**, or, &c. (§ 2).
Σ_y = **A** **S** **H** ,, ,,
Σ_z = **S** **H** (ɑ) ,, ,,

(*g*) In this complex case, as in the simpler one of § 40, each of the dialects resists the debilitations of the others, and each retreats before the resistance of the others. Although, therefore, Σ_x, with respect to Σ_y and Σ_z, wears, to some extent, the aspect of a standard system, we are not to suppose that there was, in primitive times, any necessary distinction between standard and popular dialects (§ 41 (*b*, 3)). Each main dialect was as good as the others; and such, no doubt, would have been the feeling of the corresponding sections of people, if they had thought at all on the subject.

(*h*) And although we have been principally concerned with *three* dialects only, yet those existing in the Holethnos may really have been very numerous. The number of dialectic points of difference for which Mute-variation alone offers scope is not small; and these have to be combined in different ways with variations in the other consonants and in the vowels. Our three phonetic systems should therefore be understood as characterizing three principal dialectic centres; while the dialectic areas intermediate to these centres would be occupied by minor varieties of dialect agreeing and disagreeing with the principal ones in all possible ways (§ 35 (*b*)).

45.—The question was broached in § 36 whether the phonetic affections yielding **A** and **S** originated in their respective dialects almost or quite simultaneously. Probably so: otherwise it is hard to see why the Dissimilating action between Σ_y and Σ_z assumed the direction it did. A cognate question is, whether both attacked the Cl. dialect at or about the same time. Probably they did: for if one (say the L.G. Aspiration) had attacked it any length of time before the other (the H.G. Softening), the Resistance of that dialect ought to have taken the form **H** against **A** (which it actually took); and when Reflex Dissimilation set in, the correspondent Retreat ought to have exhibited **A**, or an imitation thereof, in face of the L.G. **H**[1], whereas it actually exhibits **S**: so that the

[1] There are, indeed, some facts which seem to indicate that **A** really *did* make its influence felt upon the Cl. dialect *somewhat* sooner than **S**, and that that dialect actually began to respond in the legitimate way.

response which should have been given had there been but one debilitation involved was manifestly perturbed by the presence of a second. Such perturbation could hardly have occurred after the spread of Aspiration throughout the Cl. dialect in answer to L.G. **H**; for then the Cl. and L.G. phonetic systems would have already assumed the forms {**H** (*a*)} and {**A** **H**} respectively, and the introduction and spread of **S** in H.G. and L.G. on the existing plan would at first have given three systems of the following form :—

$$\Sigma_x = \mathbf{H} \quad (a)$$
$$\Sigma_y = \mathbf{A} \quad \overbrace{\mathbf{S} \quad \mathbf{H}}$$
$$\Sigma = \mathbf{S} \quad \mathbf{H} \quad (a)$$

where Σ_x might be written

$$\mathbf{H} \quad (a) \quad (a) \quad \ldots \quad (\mu).$$

If the Medial affection had now attacked Σ_x, it could not have modified the Tenues; for these remain to this day coextensive with **A** in Σ_y and **S** in Σ_z. We should have to suppose, therefore, that the Cl. dialect, after sending a considerable

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Those facts are to be found in Grassmann's learned article on the Aspirates (Kuhn's Z-S., xii. 80, &c.), one of the leading contentions of which is, that the L.G. dialect represents the Cl. *Hard* Aspirates by *Tenues*. This means, on my hypothesis, that Cl. **A** answered, in those cases, to L.G. **H**, and thus exactly counterbalanced a transmutation of the primitive **H** to L.G. **A**. The correspondence of those exceptional Aspirates with the *Tenues* of a commingled dialect would very well account for their preservation in Skt as Hard Aspirates. If we further suppose that the perturbing Mediæ began very soon thereafter to make their influence felt, and that the Cl. dialect proceeded to differentiate its system against *two* others, the main body of its Aspirates would have to be brought into correspondence with the L.G. Mediæ; so that we may possibly here detect the influence which determined the nature of the bulk of the Skt Aspirates, which are Soft. Both this conjecture and that with which this note started agree in implying a somewhat more active intercourse between Skt and L.G. than between Skt and H.G., also between Skt and L.G. than between the other Cl. subdialects and L.G. The Hellenic subdialect, for example, must in this case be supposed to have proceeded in a uniform line of Aspiration of the same nature as that upon which it originally entered.

division of its mutes down one line of debilitation into Aspiratæ, took the greater part of that division (§ 14 (*c*)), and transferred them to another line of debilitation, which brought them to Mediæ. Such a duplicate transmutation may not be impossible; but as it is improbable and quite unnecessary, it may be more safely put aside. The next section will offer analogical argument in favour of the opinion that if the phonetic system of any dialect had assumed the form in (μ) above, it would have maintained that form, and refused the Media altogether.

46.—(*a*) In just the same way it may be inferred that the H.G. Medial affection did not attack the Cl. dialect (properly so called) before the L.G. Aspiration. For otherwise the Cl. and H.G. systems would first have assumed the Mutually Dissimilated forms {**H** **S**} and {**S** **H**}, respectively; so that it would be necessary to suppose that, when Aspiration afterwards attacked the former, it operated upon a part of the series of Mediæ which had already been once transmuted from Tenues in a totally different way; and I have yet heard of no one who has suggested that the Mediæ could pass into genuine Aspirates. This case, however, is one of considerable interest; for although the purely Cl. system exhibits the simultaneous influence of **A** and **S**, some of the intermediate systems (§§ 35 (*b*), 44 (*h*)) maintain to this day the very form which should result from the prior action of the Medial affection. That form, as was just said, is {**H** **S**} as against the H.G. {**S** **H**}. But, after the evolution of that form, let Aspiration be supposed to spread from L.G. through H.G., and let Σ_y and Σ_z mutually adjust themselves to the forms they actually present; then all the three would constitute the following group:—

$\Sigma_{(\rho)}$ **H** **S**
Σ_z **S** **H** (a)
Σ_y **A** **S** **H**

and if $\Sigma_{(\rho)}$, while maintaining **H** against the **S** of Σ_z and the **A** of Σ_y, refused to apply a second (and indeed impracticable)

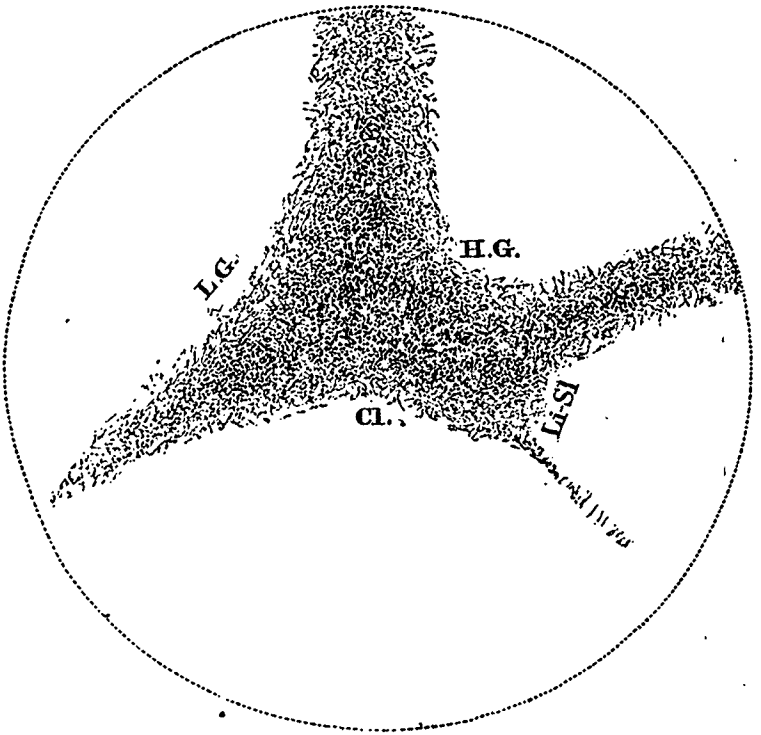
transmutation to a part of its Mediæ, it would permanently assume a form which may be written

H S S . . . . (ν),

and which corresponds to the expression (μ) in the preceding section. Now this is the very form of the Lithu-Slavonic and of the Oldest Keltic Mute-systems; and the foregoing is the plan and process whereby I would explain the evolution of these intermediate systems, in lieu of endowing them, as the theories of Curtius and others do, with a set of Aspirates which they must afterwards have completely lost. As the extreme terms of (ν) agree with Σ_x, while the mean agrees with Σ_y, we may, for convenience, represent systems of that form by the symbol Σ_{x+y}.

(*b*) But it may fairly be asked, What cause could have confined the reciprocal action and reaction of the H.G. and (say) the Li-Sl. dialects to themselves for a sufficient length of time to allow of Mutual Dissimilation between the two in respect of the two sounds **H** and **S**? Perhaps such a cause may have existed in the geographical distribution of the leading sections of the Holethnos. In order that the Mutual Dissimilation just described might be possible, we should then have to suppose that the Li-Sl. section, while in contact with the H.G. section, was at the same time comparatively remote from the L.G. section, in which Aspiration originated; so that the length of time which, under these circumstances, must have elapsed before that infection permeated the H.G. vocabulary and reached the Li-Sl. dialect, would be sufficient to allow of the latter dialect already answering, in the greater part if not all of its series of mutes, by **H** and **S** to the **S** and **H** of the H.G. dialect. This would suggest some such local arrangement of the principal I-E. tribes as that represented in the fancy sketch on page 96. And as my hypothesis requires that the dialects in which the correlative phonetic movements were going on should have been in presence of one another (that is, that the corresponding sections of people should have been in contact and even commingled), I have endeavoured to represent this condition of contact and commixture by the broad shading in the figure, instead of

drawing sharply marked lines that might indicate complete geographical separation.



(*c*) There is, however, another explanation (§ 35 (*b*)) of the form assumed by Σ_{x+y}—namely, that it might have resulted from the direct influence, or preventive Assimilation, so to say, of the L.G. dialect. The Aspirate might, indeed, have offered special difficulty to the Li-Sl. tribe; and to avoid it they might instinctively have cast in their lot with the Low Germans. This explanation is perfectly consistent with my hypothesis: it involves a non-essential point only. But the objection to it would lie in the vigorous dissimilation of the remaining sections of the Li-Sl. system—the Tenues and the rest of the Mediæ—against the L.G. Aspirates and Tenues. A dialect which could thus vigorously and distinctly dissimilate some seven eighths of its mutes might certainly be expected to make

an effort, at least, to dissimilate the remainder. But no trace of any such effort is anywhere discoverable; so that, on the whole, I much prefer the former explanation. Leaving this point, however, I remark that, in whichever of the two ways Σ_{x+y} was evolved, its phenomena, evincing both German and Classical (Aryan) influence, are singularly in harmony with that hybrid aspect of the Li-Sl. dialect which has puzzled so many of the leading philologists of the Continent, and led them to precisely opposite conclusions with respect to its affinities[1].

(*d*) The substance, then, of this section and the last is, that **A** in L.G. and **S** in H.G. broke out almost or quite simultaneously, and each existed for a time alone in its native dialect; that each of those two dialects proceeded to adopt the affection of the other, and began to do so before either **A** or **S** was adopted by any third dialect; that the two affections began to influence the Cl. dialect almost or quite simultaneously; and that Mute-systems of the Lithu-Slavonic type were moulded by Dissimilation against the H.G. system alone, before Aspiration had extended through the latter.

47.—I have now stated all I intend at present to urge towards directly indicating the cause and the manner, as they appear to me, of the evolution of the principal I-E. Mute-systems. I have tried to show that the priority of any one of the known tri-sonant systems over the others is untenable, and that the assertion of any such priority only leads to results which are opposed either to one another, or to ascertained linguistic principles. I have further argued that if no

[1] See in particular J. Schmidt, "Die Verwandtschaftsverhältnisse der I-G. Spr."; Fick, "Ehemalige Sprachenheit," &c.; Havet in the "Revue Critique," Nov. 23, 1872, and in the "Mémoires de la Soc. de Ling.," 1874; E. Förstemann, "Gesch. des D. Sprachstammes," i. 241, and in Kuhn's Z-S., xviii. 163. See also *infrà*, §§ 55-63.—Upon two general facts all are agreed, whatever difference there may be as to the explanation: viz., first, the nearer relationship of the Li-Sl. dialect to the German than to any other European tongue; and second, "the wonderful agreement whereby in Li-Sl. alone a 'Zischlaut' (Lith. *sz*, Salv. *s*) answers to the Aryan palatal Zischlaut *ç*" (J. Schmidt).

one of those systems was prior to the rest, and if, besides, they were not all coeternal, they must all have been evolved from some central parent system; and that this parent system, instead of three degrees of mutes, comprised only one degree, which could not have been weaker than the strongest degree of known mutes—namely, the Tenues. With a view to discover a motive power for such evolution, I have entered into a somewhat minute examination both of the Process of Phonetic Variation as contributory to the extension of the powers of language, and also of the distributive processes of Cross Compensation and Mutual Dissimilation, in which Language is seen exhibiting a sort of organic vitality of the very kind required for our purpose, and in the former process even more vigour than we want; and from known and less complex cases in which we can actually observe those processes in action, as well as their results, I have reasoned to the more complex case of Grimm's Law, in which results alone are before us. Lastly, I have indicated such extensions of the principles laid down as enable them to embrace phonetic systems of a different type from the trisonant systems more particularly investigated. One important consequence of the line of explanation adopted has been to bring Grimm's Law (as indeed it should be brought) into harmony with, and subordination to, the wider Principle of Least Effort; and another has been to show that all the phonetic mutation and fluctuation expressed by the Law is not confined (why should it be?) to the German dialects alone, as the current hypothesis assumes, but is distributed in diminished proportions among all the dialects correlated by the Law. There still remain two other questions without an examination of which our inquiry would be incomplete. One relates to the Exceptions to so-called "Substitution"; the other to the Historical Evidence which is supposed to justify the conclusion that the L.G. and H.G. substitutions took place in succession; subordinate to which latter question lies that relating to the apparently imperfect and irregular evolution of the H.G. system. These questions I propose to treat in order; and I shall then finally indicate by an example the way in which other obscure

problems may possibly be hereafter elucidated by the principle of Mutual Dissimilation which I have endeavoured to establish.

48.—(*a*) For the sake of simplicity and precision of treatment I have hitherto assumed that the symmetrical correspondence of the three principal I-E. phonetic systems one to another is, sound for sound, exact. This, however, is not the case; indeed the instances in which the correspondence fails are sufficiently numerous to demand a separate consideration. The wide departure of Σ_z from its ideally correct form will be treated of in conjunction with the history of that system (§§ 51–54 *infrà*). At present, Σ_z will be considered only so far as its influence may elucidate the exceptions which appear to exist in Σ_y as compared with Σ_x. These exceptions have been carefully treated by Lottner[1], whose exhaustive article is the standard authority on the subject. But these are far from all; for there is probably no leading language or dialect of the I-E. family which does not exhibit deviations from the phonetic system which properly belongs to it. Into the world of details thus opening before us it is not my intention to enter. I propose rather to establish, if possible, the general principle or principles underlying *all* the exceptions, and only to recur to individual examples for the sake of illustration.

(*b*) To test the nature of these exceptions, let us start from the regular expressions of the normal process given by formula (π) of § 29 and formula (ρ) of § 30. If Language changed with mathematical precision, those are the expressions under which the results of Fertile and Sterile Variation respectively should fall. But it does not so change; nor will it submit to be bound by artificial restrictions. Thus (1) a dialect may lose a form corresponding to forms of other dialects; and, in fact, every language of our family wants numerous words

[1] Kuhn's Z-S., xi. 161, &c. Lottner's results have been criticized and modified by Grassmann (Z-S., xii. 80: see also Appendix F, *infra*); and Dr. E. Förstemann has gone over the same ground briefly, but fully enough, in his "Gesch. des D. Sprachstammes", i. 366–372.

that are to be found in one or more of the other languages. (2) One dialect may adopt or retain a form which should have been resigned to another, while also retaining or assuming that which belongs to itself—and this, without having yet distributed the common meaning: hence it may exhibit twin or duplicate roots. A case in point is furnished by the retention, on the part of the Cl. dialect, both of the form *kar-* (*kal-*), whence Skt çi-çir-a = Zend çar-eta = Lith. *szal-u*, "cold"; and of the form *gar-* (*gal-*), whence Skt *jal-a* = Lat. *gel-u*=O.H.G. *chuol-i*=Goth. *kal-d-as*, and our "*cool, cold*," &c. So, too, the Cl. dialect appears to have maintained the triple root *mak-*, *mag-*, and *magh-* (*mah-*), but with an incipient appropriation of *mak-* to denote "length" (*μῆκ-ος*, *μακ-ρός*), one species of the bigness or greatness denoted by *μέγ-ας*, Skt. *mah-as* (but *maj*-man = majestas; where *j* = *g*), Goth. *mik-ils*, Scotch *muck-le*. This irregularity may explain the numerous duplicate roots in the individual languages, such as *δεκ-* and *δεχ-*, *τυκ-* and *τυχ-*, *λαβ-* and *λαφ-*, in Greek. (3) One dialect, while thus adopting or retaining a form strictly belonging to another, may drop or neglect to acquire its own. Thus, in Europe, the Cl. form of the first personal pronoun is based on the stem *agam* (= *ἐγώ*, *ego*, to which regularly correspond the Goth. *ik*, H.G. *ich*), whereas the Aryan (also Cl.) pronoun is based on the stem *agham* (Skt *aham*, Zend *azem*), where the aspirate indicates an apparent agreement between the Aryan and H.G. dialects[1].

(*c*) None of these cases, however, need give us much trouble, so long as one of the cognate subdialects exhibits the correct form; for it is more likely that the rest are wrong in exhibiting an abnormal form than that the one is wrong in exhibiting the normal form. But when all the subdialects of one great dialect (say the L.G.) are irregular as compared with all the subdialects of another great dialect (say the Cl.), or these as compared with those, then arise the questions, Which

[1] Other illustrations will be found in Grassmann's learned article before referred to, or in Fick's "Spracheinheit," Abhandlung iv. I purposely abstain, here and elsewhere, from piling up examples which will at once strike the reader, or may at any rate be found in books easily accessible.

are wrong? and In what way are they wrong? Now the irregularities which chiefly concern us here are those comprised under case (3), with the additional condition that all the subdialects on one side or other agree therein. But case (3) may arise from two causes: (i) the retention of a sound which should have been changed; and (ii) the changing of a sound which should have been either retained or else changed in another way[1]. For the former to occur, a dialect need only cling to the sounds which it actually possesses and has possessed from all time; for the latter to occur, a dialect must go out of its way, as it were, to be irregular: it must alter its sounds on a foreign instead of a native pattern. Although, therefore, the latter case, owing to special phonetic propensities, may be frequent enough in the later history of the I-E. subdialects, the former is much more likely to have ruled at the outset of a series of great phonetic movements. Besides, as Dissimilation would not operate instantaneously over the whole of the several dialects, but would permeate gradually through them, the divergence of those dialects may have removed them severally beyond the influence of the rest before the vocabulary of each had been completely transmuted; and in that event each main dialect would retain some few consonants unchanged. Such retention will also appear highly probable if we remember (§ 36) that each of our phonetic systems, in relation to the other two, was a system of *Resistance*,—Σ_x, indeed, almost entirely so. Now, by the hypothesis of this treatise, (1) the term "retention" can only be used of the Tenuis; and (2), as Σ_x, Σ_y, and Σ_z are supposed to have radiated symmetrically from that single sound, there is no reason why one

[1] This division seems nearly to agree with that of Dr. E. Förstemann, whose recent work ("Gesch. des D. Sprachstammes," vol. i.) did not come into my hands until long after the above remarks were written. He reduces all the exceptions under *Stillstand* ("suspension") and *Beschleunigung* ("acceleration") of the process of Substitution. By the former the L.G. (which with him is, of course, the chief sinner) exhibits the same sound as the Cl. dialects; by the latter it exhibits a sound which should properly belong to the H.G. But his reference of the other systems to Σ_x as their primitive, and my reference of all three systems to one set of parent sounds (the Tenues), will be seen to make a radical difference between our views.

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system should be more or less irregular than either of the others. Accordingly, instead of throwing nearly all the irregularities upon one system (Σ_y) and employing two precisely opposite principles[1] to account for them, I propose to distribute them, as I have distributed the phonetic changes, among all the three systems, and to show that they may be simply and uniformly accounted for by the single principle of the Retention of the Tenuis on the part of one or other of those three systems.

49.—(*a*) In order to proceed systematically, let us direct our advance by the well-known article of Lottner before referred to. This article, being based on the Chronological Hypothesis, assumes that the exceptions to the *first* "Lautverschiebung" (*i. e.* the supposed transmutation of Σ_x into Σ_y) are attributable almost exclusively to the L.G. dialect. In accordance with our hypothesis, however, we will at first vary the assumption so far as to suppose that the irregularities lie between Σ_x on the one side and Σ_y on the other, Σ_z being temporarily left aside. Then, as each of the sounds **H**, **A**, **S**, in each of those two systems, has but one *normal* representative in the other, it is clear that each sound in the one might, if all possible irregularities occurred, be *ab*normally represented by the remaining two sounds of the other. This, of course, yields six possible irregularities, making up three groups of a pair each. Thus:

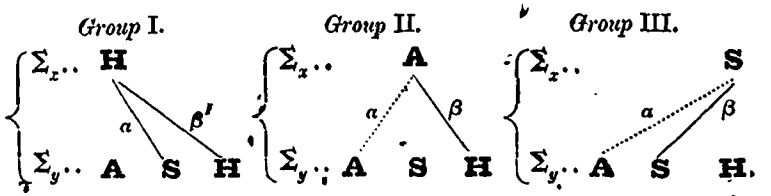
I. Instead of **A** in Σ_y for **H** in Σ_x, there might occur either **S** or **H**;

II. Instead of **S** in Σ_y for **A** in Σ_x, there might occur either **A** or **H**;

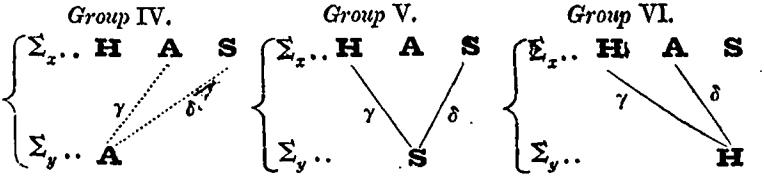
III. Instead of **H** in Σ_y for **S** in Σ_x, there might occur either **A** or **S**.

These groups of irregularities may be represented to the eye as follows, the possible irregular representatives being coupled by oblique lines:—

[1] See the preceding note.



(*b*) Now, as long as Σ_x is held to be the original standard and unchanging system from which Σ_y was *verschoben*, all irregularities (except where the Cl. subdialects differ *inter se*) are necessarily charged upon the latter system only. But if, as we have maintained, Σ_x itself enjoyed no immunity from change, but was evolved out of the one primitive series of *Tenues* on precisely the same general plan as Σ_y and Σ_z (although with much less extensive debilitation), then, in examining those irregularities, we are manifestly unable to leave Σ_x out of the account. Hence the irregularities may, with equal justice, be grouped in such a way as to make Σ_y the normal system, and Σ_x a deviation therefrom: that is to say, every pair of the mutes in Σ_x may be represented as answering irregularly to one or other of the mutes of Σ_y. This grouping may be pictured to the eye as follows:—



(*c*) Each possible irregularity, if supposed to lie between Σ_x and Σ_y only, now becomes susceptible of two interpretations: thus—

I. (*a*) Either Σ_x retains Tenues which it should have weakened to Aspiratæ;

Or Σ_y weakens (abnormally) some Tenues to Mediæ instead of Aspiratæ (V(γ)).

(β) Either Σ_x retains Tenues which it should have weakened to Mediæ;

Or Σ_y retains Tenues which it should have weakened to Aspiratæ (VI(γ)).

II. (*a*) Either Σ_x weakens some Tenues to Aspiratæ instead of retaining them as Tenues;

Or Σ_y weakens (abnormally) some Tenues to Aspiratæ instead of Mediæ (IV(γ)).

(*β*) Either Σ_x weakens (abnormally) some Tenues to Aspiratæ instead of Mediæ;

Or Σ_y retains Tenues which it should have weakened to Mediæ (VI(δ)).

III. (*a*) Either Σ_x weakens some Tenues to Mediæ instead of retaining them as Tenues;

Or Σ_y weakens some Tenues to Aspiratæ instead of retaining them as Tenues (IV(δ)).

(*β*) Either Σ_x weakens (abnormally) some Tenues to Mediæ instead of Aspiratæ;

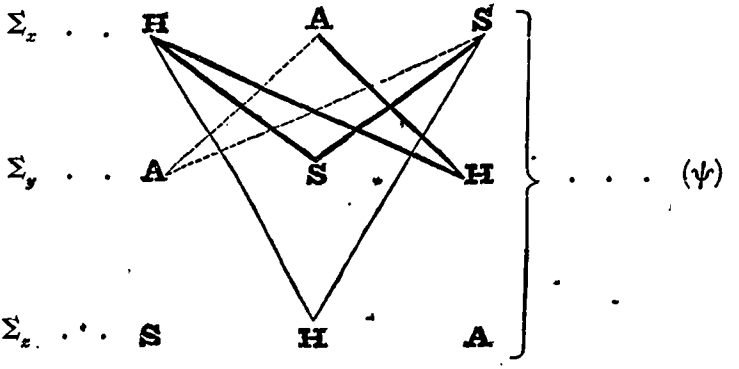
Or Σ_y weakens some Tenues to Mediæ instead of retaining them as Tenues (V(δ)).

(*d*) We have next to notice that of these *possible* irregularities those comprised under II(*a*) and III(*a*)—indicated by dotted lines in the figured groups II., III., and IV.—do not occur. If it be asked, Why, then, were they introduced? I reply, Because their absence is instructive. It shows, at any rate, (1) that Σ_x did not admit debilitation *at all*—either to Aspiratæ or to Mediæ—when it should have *retained the Tenues*; (2) that Σ_y did not admit *abnormal* debilitation to *Aspiratæ*; and (3) that Σ_y did not admit debilitation to *Aspiratæ* when it should have *retained the Tenues*. Let us see how these facts affect the four remaining pairs of alternatives. Owing to the symmetrical relationship of the systems, whatever is true of one ought, by our hypothesis, to be true of another. We may therefore conjecture, from fact (1), that Σ_y also did not admit debilitation at all when it should have retained Tenues; and fact (3) proves our conjecture correct as regards debilitation to Aspiratæ, although this is the very form of debilitation to which, by our hypothesis, Σ_y should be most prone. Fact (2) gets rid of *abnormal* debilitation from Σ_y, also in the very direction in which it was most likely to occur, *i. e.* towards the Aspiratæ. Hence the probabilities against irregular debilitation to the Mediæ on the part of Σ_y

are very strong. Still stronger is the probability against any abnormal debilitation on the part of Σ_x, the system of greatest resistance. If these probabilities are to be relied on, the only cause of irregularity now left, as between Σ_x and Σ_y, is the *Retention of the Tenuis*. And here we start with the very strong point that group I (β) (=VI (γ)) admits of no other explanation. We need have no hesitation therefore in applying that explanation, at least provisionally, to I (α) (=V (γ)) and to II (β) (=VI (δ)); under which latter case, however, there can be ranged only one pretty certain exception (viz. the Skt *grabh-*; Grk γρῖφ-ος; Goth. *grip*-an; A.S. *grîp*-an; H.G. *greif*-en) and about four doubtful ones.

(e) But this, apparently, leaves the case III (β) (=V (δ)) unexplained; for neither of the alternative interpretations of that case in subsection (c) accords with the principle of Retention of the Tenuis. Are, then, our probabilities worth nothing? By no means. The apparent discrepancy is merely an indication that we have not taken into account all the facts we ought. We have so far considered the action of only *two* systems in relation to their mutual irregularities; whereas our hypothesis requires us to consider the reciprocal action of *three*. In truth, we can no more neglect the influence of Σ_z in the production of irregularity, than the astronomer, in examining the perturbations of the solar system, can neglect the attraction of one of the larger planets. If things so unlike may be compared, each of our phonetic systems may be described, in reference to the other two, as an outlying and perturbing or attracting system, the core, the centre of gravity of which lies in the Tenuis; while the derived Aspirate and Media are but, as it were, satellites, whose perturbing influence may be taken as *nil*. Especially is this true as regards the action of Σ_z upon Σ_y, at least according to the hypothesis of this book, which provides (§§ 43, 45) that the Dissimilation between those two systems may have set in earlier than the like influence of either upon Σ_x, or of Σ_x upon either, that it may have been more vigorous, and that it may even have continued after the connexion of the German and Classical tribes had been severed (§ 53, *infrà*). Let us then introduce

Σ_z into our system of phonetic perturbations; and let us combine with it, in the following single plan, all the relationships exhibited in the above groups (I.–VI.) :—



Now let it be granted (1) that Σ_z retained a number of *Tenues* which, in order to produce perfect dissimilation *with respect to* Σ_x should have become partly *Mediæ* and partly *Aspiratæ*; and (2) that the Mutual Dissimilation between Σ_y and Σ_z was originally carried further towards perfection than that between Σ_x and Σ_z, or even between Σ_x and Σ_y. What follows from these postulates? Just this,—that to explain the case III (β) (or V (δ)), Σ_z may be substituted as the *real* perturbator in place of either Σ_x or Σ_y; and that **S** in the latter, which really represents a certain number of sounds that were wrenched out of their proper orbit, so to say, by the influence of **H** in Σ_z, is thus made to *appear* (when Σ_x and Σ_y alone are considered) as the irregular representative of **S**, and partly perhaps of **H**, in Σ_x[1].

[1] For example, the *g* of *blug-* or *φυγ-*, whence *φεύγειν* and *fugere*, is represented by *g* in Goth. *biug*-an, O.N. *bjug*-a, A.S. *bug*-an,—a case of *Stillstand*, as Förstemann would say. The doctrine of the text above, however, would attribute the irregularity to the retention of *k* by the oldest H.G. (*piok*-an), and to the adjustment of Σ_y to Σ_z rather than to Σ_x. Again, the *t* in *hort*-us, *χόρτ-ος*, is represented by *d* in Goth. *gard-i*, A.S. *gard*, our *yard* and *garden*; and this may similarly be attributed to the influence of *t* in the O.H.G. *cart-* or *gart-*, N.H.G. *Gart-en*. This form of exception runs, in some cases, through a whole series of words or inflexional forms; *e. g.*, to the Cl. passive participial termination *-ta* the L.G. answers by *-da* (except the Goth. nom. sing. in *-ths*); and here again the H.G. exhibits *t* (Förste-

(*f*) Hence, in sum, we arrive at simple and symmetrical expressions of all the cases of irregularity treated of in Lottner's article; that is to say,

(i) Σ_x maintained a number of *Tenues* which, to make that system exactly correspondent to the other two, should have been debilitated partly to *Mediæ* and partly to *Aspiratæ*; consequently its **H** is irregularly represented in Σ_y by **H** and **S** (Group I.).

(ii) Σ_y maintained a number of *Tenues* which, to make it exactly correspondent to Σ_x, should have been similarly debilitated in both ways; consequently its **H** is irregularly represented in Σ_x by **H** and **A** (Group VI.).

(iii) Σ_z also maintained a number of *Tenues* which, to make it exactly correspondent to Σ_x, should have been similarly debilitated in both ways; consequently its **H**, which attracted to itself **S** in Σ_y, is irregularly represented in Σ_x by **H** and **S** (Group V. and Scheme (ψ)).

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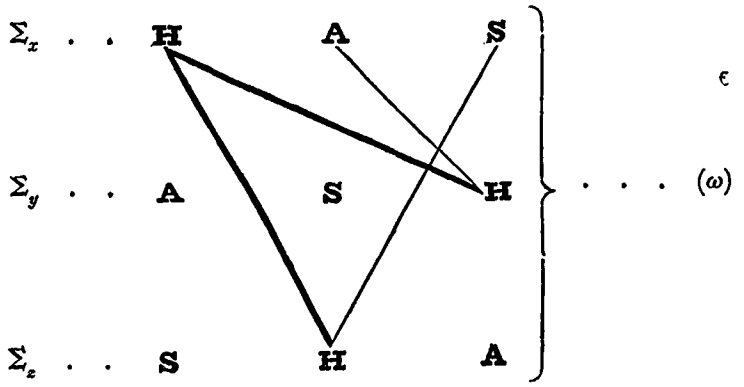
mann, Gesch., i., p. 372; Helfenstein, C.G., p. 407). Now what reason can be given, on the Chronological Hypothesis, why, on the one hand, the L.G. dialects, generally so regular with respect to the Classical, should select this line of deviation for their principal exceptions; or why, on the other, the H.G., which, on that hypothesis, is so irregular and incomplete with respect to L.G., should select the very cases in which Σ_y is irregular with respect to Σ_x for the exhibition of regularity?—It should be added, however, that the usage of the L.G. dialects is not strictly uniform. It not seldom happens that some L.G. dialects, in spite of H.G. influence, are regular with respect to Σ_x, while others have yielded to such influence. A familiar and distinct example of such dialectic disagreement occurs in the forms *fadar* and *mŏdar*; in which, if they had been regularly differentiated against the Cl. *patar*, *mātar*, þ or ð should have appeared instead of *d*; and the H.G., if similarly differentiated against Σ_z, should have exhibited *d*. But here, too, the H.G. maintained the primeval tenuis (*fatar*, *muoter* now Va*t*er, Mu*tt*er); and the more active differentiation between the L.G. and H.G. dialects led most of the former dialects (the latter remaining obstinate) to adjust themselves in these cases to the latter. Nevertheless other dialects, especially the Old Norse, appear regular with respect to Σ_x (*faðir*, *mōðir*). And it is not to the passage of *d* into ð, but to the existence in this country of dialects closely related to the O.N., that I would attribute the somewhat recent appearance of the spirant in our "fa*th*er" and "mo*th*er" (See Appendix D). Compare the instructive difference in the case of Cl. *bhrât-ar*, L.G. *brōth-ar*, in relation to O.H.G. *pruod-ar* = *Bru*d-*er*, the last being regular with respect to the first.

Thus the simplest summation of the whole series of irregularities is represented to the eye in the following scheme:—



50.—Remarks on the foregoing section.

(*a*) It will be seen upon an examination of Scheme (ω) that Σ_x and Σ_y, and likewise Σ_x and Σ_z exhibit a number of Tenues in common; for to say, as in expression (i) above, that **H** in Σ_x is represented by **H** in Σ_y,—and, as in (ii), that **H** in Σ_y is represented by **H** in Σ_x,—is saying the same thing. And similarly of Σ_x and Σ_z, as correlated in expression (iii). The nature of the case precludes distinct evidence as to the share contributed by each system to each little common stock. It is not impossible—indeed, on the hypothesis of this book, it is very probable, that, in each case of irregularity the whole may be due to Retention of the Tenuis on the part of one system only. At any rate, such a distribution of the exceptions (scheme (ω)) would exactly fit in both with the relative mutability and with the direction of change which we have assigned to the several systems. Expression (i) would still stand as it does; for Σ_x being the system of greatest resistance, and neither **A** nor **S** being native to it, the Tenues which it exhibits in common with Σ_y and Σ_z would then be entirely due to retention on its own part, and in no case on the part of the latter systems. Consequently, in (ii) it would suffice to say merely that Σ_y retained one or more Tenues which should have been debilitated to Mediæ (the characteristic debilitation of Σ_z); and in (iii) that Σ_z retained Tenues

which should have been debilitated to Aspiratæ (the characteristic debilitation of Σ_y). In short, the resistance of each system was exerted (as might be expected) against that sound or those sounds which were foreign to itself.

(*b*) In Group V. the L.G. **S** appears as representing both **H** and **S** of Σ_x. The former irregularity has just been assigned to the retention of the Tenuis by Σ_x, and the whole of the latter irregularity to the retention of the Tenuis on the part of Σ_z. Hence one might conjecture, independently of other reasons (§ 45, *note*), that the L.G. had more influence than the H.G. upon the Cl. dialect; also that the mutual action of the two German dialects was at one time more vigorous than their action against any third system, and that it was continued to a later period. For the simplest way of explaining the extensive representation of the **S** of Σ_x by **S** in Σ_y will now be to suppose that Σ_x first adjusted its Mediæ to the Tenues of Σ_y, and that some of these Tenues were subsequently debilitated to Mediæ by way of adjustment to the more obstinate Tenues of Σ_z, which latter Tenues, in order to effect exact correspondence between Σ_z and Σ_x, should, if the mutual action between these two systems had been sufficiently strong, have been weakened to Aspiratæ. It is manifest that Σ_x could not again follow Σ^y; for that would have involved the impossible change (§ 46 (*a*)) from **S** to **A**.

(*c*) It will now be obvious that the fact adduced by Lottner in support of Curtius's hypothesis (§ 12 *ante*)—viz., that the substitution of the L.G. **S** for the Cl. **A** is more thoroughly executed than either of the other substitutions—is susceptible of a totally different explanation. This fact, indeed, is to a great extent explicable by the very small part played by the Aspirates in the Cl. Mute-system (§ 14), which of course leaves room for proportionately few exceptions. That the proportion is still smaller is due to the position which **A** in Σ_x and **S** in Σ_y hold in relation to **H** in Σ_z (Schemes (ψ) and (ω)), and to the dissimilating effect of the latter upon Σ_y (§ 49 (*e*), and (*b*) above), whereby not only the Mediæ of the L.G. system corresponding to the Aspiratæ of Σ_x are kept in their places, but others also are added to their number.

Yet even this powerful influence is counteracted by that of **H** in Σ_y. For, be it observed, where irregular representation of the Cl. **A** *can* occur as the result of *Retention of the Tenuis* on the part of Σ_y, there it *does* occur (Group II(β)). Where it could only occur (Group II(a)) as the result of *irregular debilitation* on the part either of Σ_x or Σ_y, there it does *not* occur. That is to say, in accordance with our former interpretation of the facts, Σ_x did not admit debilitation to **A** when it should not, nor did Σ_y admit debilitation to **A** in lieu of debilitation to **S**. Or, from another point of view, the Media of Σ_z was powerless to wrench any of the mutes of Σ_y out of their places.

51.—(a) The more intimate relationship between the two German systems which I have tried to demonstrate has given rise to facts which have been diverted to the support of the Chronological Hypothesis[1]. Some of these, viewed apart from our general scheme, appear to fit in well enough with that hypothesis, so far as it assumes the direct evolution of Σ_z out of Σ_y. But with this interpretation of the facts are combined arguments drawn from the apparently imperfect evolution of the H.G. system; for it is on this ground that Σ_z is generally held to be a comparatively modern and incomplete variation from Σ_y, from which (we are told) it was unable to differentiate itself more than partially, with which it preserved a great deal in common, and to which it has manifested a strong tendency to return[2]. But here the only statement that is approximately true is the last: the others are merely inferences from phenomena which may be interpreted in a precisely opposite way. For there are *two* stages in the

[1] "Le haut-allemand se plie aux exceptions du gothique, et c'est en prenant celles-ci pour point de départ qu'il fait son évolution propre. Par exemple, lorsque, par une dérogation sans motif appréciable, la racine védique *grabh* ('prendre') se présente en gothique sous la form *greipan* au lieu de *greiban*, le haut-allemand *greifen* a opéré la substitution en subissant l'influence de *greipan* et non celle de *grabh*, qui aurait donné *greipen*. C'est donc au gothique qu'il est subordonné."—Baudry: G. C., pp. 148, 149. (But see the preceding note.)

[2] Baudry: G. C., p. 148; and Peile: "Grk and Lat. Etym.," p. 157.

history of commingled dialects at which the phenomena referred to may present themselves—namely, not only when such dialects are passing from absolute identity, through mere commixture and contact, onward towards complete separation, but also (§ 39 (*d*), *ad fin.*) when, before separation, there arise external causes of sufficient potency to counteract and reverse their tendency to divergence, and to weld them again into one. It is important, therefore, to inquire whether the Mutual Dissimilation between Σ_y and Σ_z may not have been at one time much more completely executed than it appears to have been in the eighth or ninth century, so far as the oldest remains of the H.G. dialect enable us to reconstruct its mute-system at that period; and whether the extensive and violent changes of a social and political nature among the Germans, High and Low alike, may not, even within our era, have destroyed the phonetic equipoise upon which the maintenance of the completely dissimilated mute-systems depended, and have produced a partial, but now arrested, re-assimilation. Questions of this kind require that we should further ask what and what sort of external evidence can be adduced in support of the successive phonetic evolutions assumed by the Chronological Hypothesis. I will consider the latter question first.

(*b*) The small amount of external evidence discoverable was originally collected by Grimm himself; and, indeed, it is his interpretation thereof which really constitutes the Historical Hypothesis of the Law which goes by his name. His object, of course, is to show that there was a time when the L.G., and a still later time when the H.G., consonantal stage had not yet been reached. The former of these periods he brings up to the middle of the first century of our era[1], and

[1] At least for the eastern Low Germans; for he remarks (Gesch., p. 483):—"Was uns von deutschen eigennamen bei Cæsar, Plinius, Tacitus, überliefert worden ist, hält in der regel den gothischen consonantismus nach der ersten lautverschiebung fest"; and then follows a list of nearly forty such names. E. Förstemann (Gesch., i. 356), with good reason, throws back the supposed *Verschiebungen* to an indefinite antiquity:—"Wir behaupten, dass Tenuis zur Spirans [the *latest* of the three substitutions on Curtius's hypothesis, which F. adopts] bereits in so

the latter up to the seventh century. But after a careful consideration of all that he urges, I confess myself unable to find in it any justification for such bold and precise conclusions. Instead of constructing his hypothesis out of facts, he seems to me to deflect facts into subordination to a preconstructed hypothesis. Indeed most of the supposed evidence he quotes seems to me to point towards totally different conclusions.

(*c*) To prove the absence of change from the mute-system of the (L.G.) Getæ and Daci in the earlier part of the first century, Grimm relies chiefly on the once popular work of Dioscorides upon Materia Medica. In that work Dacian equivalents are given for the names of some thirty-two plants; and of those Dacian words six are considered by Grimm to exhibit still "unverschoben" mutes, viz.:—(1) *κρουστάνη, krūstanē*, equivalent to the Greek *χελιδόνιον*, which Grimm connects with the Lithuanian *kregždė*, "swallow"; (2) *ἄπρους, aprūs*, equivalent to the Greek *ξυρίς*, for which he interrogatively suggests a connexion with the Latin *aper*; (3) *δύν, dyn*, equivalent to the Greek *ἀκαλήφη*, and to the Latin *urtica* or "nettle" (the Welsh is *dyn-ad*); (4) *πριαδήλα, priadēla*, equivalent of *ἄμπελος μέλαινα*, and related to the Old High German *friedela* = "amica"; (5) *πεγρίνα, pegrina*, another name of the same plant, which Grimm assumes to be related to an arbitrary Low German *fagreina*, "pretty one," from the L.G. *fagr-*, A.S. *fægr*, our "fair"; (6) *δοχελᾶ, dochelā*, equivalent to the Greek *χαμαίπιτυς*, "ground-pine," which is made a case in point by a "verschiebung" into Gothic *tagl*, "hair, tail," only the meaning cannot be made to suit the plant ("nur weicht geschlecht ab und schilderung der pflanze," as Grimm naïvely adds). In these words the *k, p, d, p, p*, and *d* are supposed to have been still "unverschoben" from the Cl. stage. But as it will be necessary to examine in detail some of Grimm's explanations both of these and of

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alter Zeit verwandelt wurde, dass selbst unsere ältesten Namen keine Spur mehr von der alten Tenuis aufweisen." This is, of course, so much to the advantage of the views put forward in this Treatise.

other words, and as this can best be done by analysing and examining the whole list, I shall discuss these six when they come before us in their proper order.

52.—The said list then may be divided into several groups, as follows[1]:—

(*a*) Words useless for our purpose: as *ὄρμια, σαλία, ὅλμα,* which contain no mutes; *σκιαρή,* where the *κ* is "protected"; *μόζουλα,* "thyme," which Grimm connects with our *moss,* treating the *ζ* as *s* (see *ζουόστη,* in (*d,* 2, ii.) below); *ἀνιασσεξέ* (see (*d,* 3, ii., *note*)), and *κυκωλίδ-α,* which, if native and connected with "cuckoo," may still exhibit the same consonants as (*e.g.*) the Latin *cuculus,* inasmuch as onomatopœias escape "verschiebung" so-called.

(*b*) Borrowed words: as *βλής* (=Grk *βλῆτον*), *ὄρμια* (=Grk *ὄρμινον*), *μαντεῖα,* and perhaps *βουδάλλα* (=Grk *βούγλωσσον*) and *γονολῆτ-α* (=*λιθόσπερμον*), all apparently from Hellenic sources; *προπεδουλά* is, no doubt (as Grimm suggests), a mere error for *πεμπέδουλα, pempedula,* the Gallic name (equivalent to the Greek *πεντάφυλλον,* or Latin *quinquefolium*); to which should be added *dyn;* also apparently derived from a Celtic source (§ 51 (*c*)). In fact, although this word *dyn* is one of the six pillars of Grimm's hypothesis, he is nevertheless so uncertain about it as to suggest (Gesch., p. 217) the reading of *Γάλλοι,* instead of *Δάκοι,* for the name of the people to

[1] Throughout this examination we must continually bear in mind three circumstances: (1) the popular corruptions of names of common plants, which are often extensive and grotesque,—*cf.* our *dandylion, sparrow-grass,* fever-*few,* &c.; (2) the probable introduction of errors in the successive transcription of ancient MSS., even where the original author may himself have been strictly correct (and here the printed editions of Dioscorides, which alone I have been able to consult, give us no assistance); (3) the propensity of the Greeks to give an apparently Grecian cast to foreign words by slight modifications of the spelling; *e.g.,* *Ἱερο-σόλυμα* for Jerusalem, as if from *ἱερός*; *Λευκανία,* as if from *λευκός* (See an article—"Die Veränderung lateinischer Eigennamen im Griechischen"—by Dr. F. Strehlke in vol. i. of Kuhn's Z-S.). After making due allowance for these qualifying circumstances, we must esteem ourselves fortunate if we discover that a moderate percentage of the words in the List still enable us to assign them their parentage with an approach to certainty.

whom it belonged. To the words borrowed from the Greek he would add *κερκεραφρών* (= Grk *ἀναγαλλίς*), as if from *ἄφρων*; but *κεραφρων* is so suspiciously like the Latin *cerebrum* (*-b*rum for *-bh*arum), that I can hardly help attributing the word to a source more nearly akin to the Italian dialects: the initial *κερ-* then becomes a mere reduplication. To a similar source (Aedic?) should probably be assigned *κοαδάμα*, where the *κοα-*[1] is almost certainly truncated from *aqua*, and *δάμα* is perhaps = "dweller" and akin to *domus*: compare the Greek name for this very plant (*ποταμο-γείτων*).

(*c*) Words hitherto quite undetermined, and perhaps undeterminable; as *κοτίατα* (=*gramen*), *διέλεια* (=*ὑοσκύαμος*), and *καροπίθλα* (=*κατανάγκη*=*herba filicula*).

(*d*) Words that have a distinctly German aspect. These may be arranged in three clusters:

(1) *Low German.*—(i.) *πόλπους* = Grk *ἄνηθον*. Grimm rejects any relationship between this *polpus* and *βολβός*, on the ground that the name does not suit "dill": but this argument is by no means decisive. The onion does not seem to have been designated among any of the Germans by an equivalent of *bulbus*; yet the latter name, even if not preserved by them from primitive times, was very likely, from its commonness, to be caught up from their neighbours, and might easily be applied to any plant (whether closely resembling the onion or not) for which a native name was wanting. The case is precisely similar to that of the well-known Gothic form *ulbandus*, which is only the word (*ἐλέφας*) *elephantus*, phonetically transmuted, and applied (not to the elephant, but) to the *camel*.

(ii.) *ῥαθίβιδα*=*ἀστὴρ ἀττικός* or *ὑόφθαλμος*. Grimm himself suggests the O.N. (*i. e.*, observe, a L.G. form) *raðabið* =*"tempus consultandi"*. But as both of the Greek names indicate a circular flower, his other suggestion that *ῥαθι-* represents a L.G. equivalent of the H.G. *rad*=Lat. *rot-a*, seems to me preferable, even though the other half *-βιδα* should then remain unexplained. But as to this half, if a very likely

[1] *κο-* is one of the regular Grk representatives of the Lat. *qu-*; *e.g.*, Κοΐντος=Quintus. See Strehlke's article just now referred to.

error in transcription (B or β for P or ρ, which might even be followed by δ for ϑ) might be suggested, we should arrive at the musical duplication *rathirida* (or raðiriða) = "wheelikin". However, there are German compounds still extant which approach very nearly to the word as it stands; *e. g.*, the A-S. name of a *beetle*, *hræð-bita* ("quick-biter"), equally applicable to a plant, if of pungent taste.

(2) *High German.*—(i.) κρουστάνη, *krūstanē*, equivalent to the Greek χελιδόνιον μέγα. This is Grimm's first example, and one on which he appears to place special reliance[1]. The Græco-Italian word χελῑδον- (for χερε(ν)δον- = *hiru(n)don-*) seems, as a name for the bird, to have been supplanted, among the Germans, by varieties of our word "swallow". But, as Grimm's remarks imply, the plant, among the Dacians as well as among the Greeks, might (not "must") have been designated after an older name of the bird. What he does not notice is that the Dacian name is not merely equivalent to, but nearly identical with, the Grecian, if only the consonants it involves be supposed *at the H.G. stage*. The Cl. stem nearest in form to the Dacian is the Lat. *hĭru(n)don-*; the L.G. form corresponding to this would be *girū(n)tan-*, *gĭrūtan-*, or *grūtan-* (the nasal being lost, as in the Greek); and the ideally correct H.G. form would be *kĭrūtsan-* or *krūtsan-*. The internal *ts* (ʒ = ʒ), which probably in ancient as in modern times, represented the H.G. approximation to the dental aspirate, would be a harsh and strange sound to Ionian ears, and would, even if correctly written by the original author, be almost certainly improved to στ in transcription. I suggest therefore that *krūstanē* is intended for *kirūtsanē*, an ideally correct H.G. representative of *hiru(n)-don-* and χελῑδον-. (See also the following word.)

(ii.) ζουόστη = Grk ἀρτεμισία = Lat. *valentia*. Here Grimm considers ζ = *s* (as in μόζουλα, group (*a*) *suprà*) and ζουόστη

[1] On the ground that it represents and is represented by the Lith. *kregždė*. But there can surely be no connexion between them: neither the stems nor the suffixes appear to be the same. Accordingly, Fick (W-B., 204 and 615) connects *kregždė* with a root (*s*)*kark-* or (*s*)*karg-*, and (p. 69) χελιδών with a root *ghar-*.

therefore = *swôsta*. This he takes to be a contraction of *swêsosta*, "gratissima, familiarissima," the superlative of the Goth. *swês*, A-S. *swæs*. This is not impossible, certainly, though the quantity of the *ô* is against it; and, if correct, it would remove the word to group (*a*) above. Let, however, *στ*, as we have supposed in the preceding example, represent the O.H.G. dental aspirate, and the derivation becomes much simpler; for *swotsē* (*swozē*) is then the O.H.G. *suozi* or *suazi*, M.H.G. *süeze* = N.H.G. *süss*, and our own *sweet*.

(iii.) *δοχελᾶ* = Grk *χαμαίπιτυς*. Here Grimm himself quotes the A-S. word *þäcele*, "fax, lampas," as apparently related; but since his hypothesis requires him to prove, if possible, that in *dochelā* the mutes are still at the classical stage, he neglects to draw the obvious conclusion from the close correspondence of the two. The meaning of the A-S. word is probably a secondary one (*cf.* the Lat. *taeda*), although even this meaning would be apposite enough in reference to a bright yellow flower (quasi *taeda* vel *fax pratorum* = *λαμπὰς λειμώνων*). But the Grk name suggests that its equivalents in cognate languages would perhaps involve the notion of "pine"; and this may easily have been the original meaning both of *dochela* and of *þäcele*, as it was of *taeda*. May perchance the Latin *taeda* itself be for *taega*, and may the two German words therefore represent a possible Latin diminutive *taegula* = *taedula*, "low or small pine"?

(iv.) *ἄπρους* = Grk *ξυρίς* = Lat. *gladiolus* vel *iris agrestis*. Grimm would connect this *aprus* with Lat. *aper*; but if we take the word as H.G., the Gothic gives us in the word *abras*, "strong," an exactly corresponding L.G. form, akin to the Cl. root *abhra-* (whence *ὄβρι-μος*). Such an epithet is suitable enough for the fragrant sweet iris, and especially so for the *iris foetidissima*.

(v.) *σέβα* = Grk *ἀκτή* = Lat. *sambucus*: "*καλαμοειδεῖς ἔχουσα κλάδους*" is the description: "O.N. *sëf*, 'scirpus, juncus;' Swed. *säf*; Dan. *siv*; O.H.G. *semid, semida*, 'carex,' for *sebid, sebida*: in Austria *sebde* still occurs beside *semde*." This quotation is from Grimm; who thus himself represents *σέβα* to be H.G., and nothing else.

(3) *Mixed* or *Doubtful.*—(i.) τουλβηλά = Grk κενταύριον = Lat. *febrifugium.* Grimm connects *tulbela* with the L.G. root *dalb-*, A-S. *delf-an,* our *delve.* O.H.G. *bi-delb-an* or *pi-telp-an*; but, bent on proving the Dacian mutes to have been *unverschoben,* he would read θουλβηλά. No need: the derivation being granted (*faute de mieux*), the initial *t* is H.G., like the consonants of the five preceding examples; and the internal *b* is either accounted for by the adjacent liquids, or perhaps it regularly corresponds to the *f* in the A-S., which may really represent the correct L.G. form.

(ii.) σικουπνοέξ = Grk ἠρύγγιον. *Sikūpnoex* seems beyond satisfactory restoration. Grimm's O.H.G. *sigufnâst,* "victoriæ flatus," and *zigâ-fahs,* "goat's-beard" are hardly admissible; they introduce fresh incongruities, and only account for one consonant after all. As, however, *o* in Grk sometimes represents *u*=*w* (See κοαδάμα in (*b*) *suprà*), -οέξ is perhaps akin to the L.G. root *wahs* or *wax,* and therefore=N.H.G. (*ge-*)*wächs.* Further, Dr. Donnegan, who was a medical man, and gave considerable attention to Grk botanical terms, connects the Grk name ἠρύγγιον with ἐρεύγ-ομαι, on account of the supposed properties of the plant. If he is right, then, correspondingly, under σικου-, we may perhaps detect the L.G. *siuka-*, O.H.G. *siuh,* our *sick.* The coalition of π and ν may be attributed to the Greek propensity adverted to in the first note to this section. The ἔκθλιψις of an internal short vowel would offer an opportunity, not to be neglected, of Græcizing the aspect of the word by effecting a fallacious assimilation to πνοή. Supposing such vowel to have been *ă,* then, for the central component, we may guess at the O.H.G. *pano-*, A.S. *bana,* our *ban* and *bane.* Or the π may itself be put for a Dacian *b*; for I shall shortly show other reasons for thinking that Dioscorides has sometimes represented the hard German *b* by π. If this were the case here, the whole compound *siuka-bana-wäx* would be L.G., and="sick(man's)-bane-plant" (*cf.* wolf's-bane, rat's-bane, hen-bane),—the *wäx* being a pleonasm[1].

[1] In ἀνιασ-σεξέ (group (*a*)) I suspect that -σεξέ is but a corrupted inflexion of the above -οεξ, Ἀνιασ- has a strikingly German aspect; but

(iii.) Δάκινα = Grk λειμώνιον = Lat. *veratrum nigrum* or *tintinnabulum terrae*. Grimm himself refers the word to the L.G. *daga-*, O.H.G. *tak-*, "day" (*cf.* our "daisy"). In that case, the *d* is L.G. and the *k* is H.G.; but this is not fatal to the derivation, on account of the extensive phonetic mixture in many of the German dialects: or, still better, as *π* for *b* in the preceding word, so here *κ* may have been written for the hard German *g*, and then the whole word is L.G.

(iv.) Τουτάστρα = Grk κολοκυνθίς = Lat. *cucurbita sylvatica*, or wild gourd. "Lässt die runde kürbisgestalt an ahd. *tutto* denken?" asks Grimm. Very likely; his objection to it is, of course, its chief recommendation to us; viz., that *tutto* is H.G.

(v.) φιθοφθεθελά = Grk ἀδίαντον = Lat. *cincinnalis* = our "maidenhair". I had originally relegated this appalling word to group (*c*); but its exuberance of aspiration (§ 14 (*c*, *d*)) seems to me to indicate that the word it is intended for must have been a L.G. word. On the other hand, the combination *φθ* is a distinctly Grecian one; yet in this case the insertion of the *θ* is probably due to the influence of the preceding and following *θ* upon the ear of transcribers who were already only too prone to Græcize their spelling. Striking out this *θ*, we have left φιθο-φεθελά, *fitho-fethela*,—an unmistakable approximation to a reduplication (*cf.* "*rathibida*," (1, ii.,) and κερκεραφρών in (*b*) above), based, I would suggest, on the L.G. form *fithra* or *fethera* = our *feather* = H.G. *feder*. A name so derived would at any rate be admirably descriptive of the plant.

(vi.) προδίορνα = Grk ἑλλέβορος μέλας = Lat. *veratrum nigrum*. Here *-orna* is a mere termination. For *prodi-*

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its connexions are quite uncertain. ΑΝΙΑΣ- may indeed have been easily corrupted from ΑΜΑΣ, or even from ΑΜΙΑΣ; and in A-S. *amas* denotes a weaver's rod (the plant was rushy); and *ameos* is already the name of a plant, perhaps bullwort; which again brings to mind the umbelliferous genus *Ammi*,—a name which occurs in Pliny. The Grk name of the plant, however, was ὀνόβρυχις, the smell being supposed to "make asses bray" (Donnegan); so that some mutilation of ὄνος or *asinus* (or a German equivalent) may lie hidden under ἀνιασ-.

Grimm refers to Goth. frôþs "sapiens" (although the quantity of the ô is against it); so that *prodi-orna* would be "das klug machende"—an ingenious allusion to the employment of hellebore among the Greeks and Romans for the cure of insanity. Whether the Dacians similarly used the plant we do not know. But if this derivation is correct, the *d* is in the H.G. stage, and the *p* apparently in the Cl. The question therefore arises, Which letter shall be adjusted to the other? Grimm's mode of treatment requires the *d* to be considered an error for a Cl. *t*. But as we have not yet found in this list of words any other clear example of Cl. consonants, while many German ones have presented themselves, we are bound, I think, on the contrary, to regard the *d* as at the H.G. stage, and to adjust the *p* thereto. It seems, however, at first sight, difficult thus to reduce the *p*; for, as is well known (§ 16 (*c*)), where the L.G. exhibits *f*, the oldest *extant* H.G. generally exhibits *f* also; hence the L.G. *frôþ-* would, in the known forms of O.H.G., appear as *frôd-*. I must here confess, however, that I propose, in the sections next ensuing, to show (what is in fact an essential part of my hypothesis, namely) that there was a remoter time, behind that of the oldest known H.G., when the ideally correct H.G. forms with initial *b* and *g* (like those with initial *d*) were actually in use. Our difficulty here will then be reduced to a much smaller one—viz., that *π* occurs where we should expect *β*. But this may be (1) because the Greek *β* (which often represented the Latin *v*) was too soft and unsettled a sound to represent the firm German *b*, and hence (See (ii) and (iii) above), *π* may here have been employed for that purpose: or (2), still more probably, the *π* may simply be due to the unconscious assimilation, on the part either of the author or of a transcriber, of the first syllable to the Greek preposition *προ-*, as in *προ-πεδουλά* (See (*b*) above). I should take the word therefore, if derived as Grimm suggests, to be really *Brod*-iorna; where *brod-* is the primitive and ideally correct H.G. for the L.G. *froth-*. It is not impossible, however, that *prod-iorna* may really involve reference to a decoction made of the plant for medicinal purposes; in which

case *prod* is the O.H.G. equivalent (actually extant) of the O.N. and A-S. broð = our "broth".

(vii.) πριαδήλα = Grk ἄμπελος μέλαινα: apparently a sort of wild vine. This word offers, perhaps, a clearer case of relationship to a German word than any other in the whole list. The oldest extant H.G. related word is of the form *friedila*, "amica," which agrees with the O.N. *fridill* as to its consonantal stage. The related Cl. forms exhibit *p* and *t*; as Skt *pre*tar = O.Sl. *prija*teli, "amicus"; so that the L.G. forms should exhibit *f* and *th* (*cf.* the kindred words Skt *priya*tva = Goth. *frija*thva), and pure H.G. should exhibit *b* and *d*. Consequently (excluding the notion of Lithu-Slavonic influence) we meet with precisely the same phenomenon in *pria*dela as (according to Grimm's view) in the foregoing *pro*diorna; *i.e.*, the internal consonant is at one stage, the initial at another. To harmonize the discrepancy I would again, for the same reasons as before, make the π = a hard *b*, and read *Briadela* as the primitive H.G. form, the *b* of which in later times was assimilated to the L.G. *f*. (See §§ 53, 54.)

(viii.) πεγρίνα: another name for the plant designated by the preceding word. Grimm refers us to the Goth. and O.N. *fagr-*, A.S. *fægr-*, (our "fair,") and the O.H.G. *fagar*. Here again the inner consonant (*g*) is properly L.G.; but by the influence of the *r* would become H.G. also; while the initial *p* is to be supposed "unverschoben," or arrested at the Cl. stage. This is the fourth or fifth discrepancy of the kind; so that, if πεγρ- really represented *fagr-*, I should say once more that π is for a hard β; and that *begr-* represents the primitive H.G. stem. But the connexion is far from indubitable, especially as it gives a second (and somewhat inapplicable) figurative name to the same plant. I am rather disposed to regard this name as a literal and concrete one, which alludes to the fruit or "berries" of the plant. In most of the old German words for "berry," as the Goth. (*basja*), O.N. (*ber*), and O.H.G (*peri* and *beri*), no *g* indeed appears; but the A-S. is *berie* or *berige*, which means "grape" as well as "berry"; and the genitive plural is *berigena* or

bergena. Now we have already seen a remarkable coincidence or two between the Dacian and the A-S., as in the case of δοχελᾶ = A-S. þäcele (2, iii), and perhaps of τουλβηλά = A-S. *delf-an* (3, i). Possibly, therefore, in the present case likewise πεγρίνα may be (by an easy and probable transposition of P and Γ[1]) for *perg*-ina, a H.G. form exactly equivalent to a L.G. *berg*-ina, *i. e. berig*-ina = "grape- or berry-plant." The appropriateness of such a connexion is obvious. It should be added that Bopp[2] traces the word "berry" to the Skt *bhaksjam* (*i. e. bhag-s-ja-m*[3]), akin to the Gk φαγ-, which accounts for the guttural, although it introduces irregularity on one side or the other.

(*e*) Thus, with fewer and less violent assumptions than are necessary in order to reduce the mutes of six refractory words to the Cl. stage, we have found, I think, *two* (πόλπους and ῥαθίβιδα) which are distinctly L.G., *three* which are probably so (σικουπνοέξ, δάκινα, and φιθοφθεθελά), *five* which are almost certainly H.G. (κρουστάνη, ζουόστη, δοχελᾶ, ἄπρους, and σέβα), and *five* which are less certainly so (τουλβηλά, τουτάστρα, προδίορνα, πριαδήλα, and πεγρίνα). The very words, therefore, by whose aid Grimm proposed to prove that the L.G. consonantal stage had not in the first century been reached among the Dacians, seem to have turned traitors, and to show, if they show anything, not only that L.G., but even, and still more (*mirabile dictu*), that H.G. sounds were prevalent among that people.

53.—(*a*) This, of course, is completely at variance with Grimm's proposition that the H.G. phonetic system was not evolved until the seventh century after Christ. It behoves

[1] Transposition is almost a characteristic of the Greek language within itself. Thus ξ is often = σκ, ψ = σπ, ζ = σδ; stems in αιν-, αιρ-, &c., *plus* a vowel, are for older forms *αν-ya*, *αρ-ya*, &c. The form *begera* (=*berigena*), however, in which *g* precedes *r*, actually occurs in A-S. (See Bosworth).

[2] V. G., iii. 343 (on the suffix *-ya*):—"Von Substantiven gehört der Neutralstamm *basja* 'Beere' hierher, wenn es, wie ich vermuthe, dem Skr. *b'áks'-ya-m* 'Speise,' eigentlich 'zu essendes' entspricht (von *b'aks'* 'essen,' Gr. *φάγ-ω*) und des Gutturals der Wurzel verlustig gegangen ist."

[3] Fick, W-B., p. 133.

us therefore to examine the additional arguments by which he attempts to support his proposition. The chief of them amounts merely to this,—that no traces of that system are anywhere to be found[1]. An obvious reply is, that non-appearance and non-existence are very different things, and that to reason from one to the other is quite fallacious, unless the reasoner is prepared to assert his own omniscience. As regards the case before us, any such assertion would be strangely inconsistent with the scantiness of our information respecting the early Germans, and especially respecting their language. Here is a numerous and powerful cluster of tribes that early in our era spread over half Europe; and yet little remains, beyond a score or two of proper names, to shed light upon the texture of their speech. Who knows what diversities of dialect prevailed, not only among these scattered tribes, but even among different sections and strata of one and the same tribe[2]? Such an argument as the one just referred to can only, at the best, hold its dubious footing until the smallest piece of distinct evidence to the contrary is producible. Such evidence, I think, is furnished by the Dacian terms as explained in the preceding section; and it certainly seems to me that the accumulated force of all of them together is not easily to be evaded.

(*b*) Two points in connexion with those words may perhaps offer a difficulty:—first, the fact that, for some of the foregoing explanations to hold good, certain consonants must have been *ideally correct H.G.* forms rather than the *really L.G.* forms which now to a great extent characterize H.G. This, however, I hope almost immediately to show to be in strict harmony with the probabilities of the case. The other point is the *mixture* of H.G. and L.G. words among the same people;

[1] "Niemals aber erscheinen die laute der zweiten, *d.i.* ahd. verschiebung, *die also* im ersten jh. *sicher nicht entfaltet war.*"—Gesch., p. 483.

[2] "Von den ersten Anfängen der geschichtlichen Zeit an hat es in Deutschland eine Menge stark ausgeprägter Dialekte gegeben, von denen jeder in einem beschränkten Bezirk Alleinherrscher war, und ebenso viel, richtiger ebenso wenig, Anspruch auf den Namen der Deutschen Normalsprache hatte als alle übrigen."—Whitney (Jolly), p. 242.

for while the bulk of those words apparently exhibit H.G. consonants, a few apparently exhibit L.G. consonants. But, remarkably enough, the earliest extant specimens of O.H.G. present unmistakable traces of a similar mixture. The examples from the Prayer of Wessobrun and the Song of Hildebrand, and the curious rules of Notker, are well known. Grimm's interpretation of this phenomenon is that in the ninth century the H.G. substitutions were not yet *completed*; and, asks Baudry triumphantly, "des faits de cette nature [*e. g. dat* for *daz*] ne tranchent-ils pas la question"?[1] By no means. That interpretation, as has been said (§ 51 (*a*)), is only one of two of which those facts are susceptible. Instead of indicating *incomplete evolution* of one dialect from another with which it was previously identical, they may indicate *incomplete coalescence* of two dialects that were previously quite distinct.

(*c*) The supposition (to treat of this first) that Σ_z was *less completely* evolved from Σ_y than Σ_y from Σ_x, is arrived at by combining what is *known* of the H.G. Mute-system since the ninth century, with what is *imagined* about it before that time. All we can rely upon, however, is what we know; and that is, the recent and partially-executed tendency of Σ_z to assimilate itself to Σ_y. In popular views on the subject that tendency is sometimes represented as innate and spontaneous, and (like the original evolution[2]) as the result of some inherent necessity. And so the supposed original movement of Σ_z away from Σ_y and its later movement back again towards Σ_y, although exactly opposed to each other, are both, it seems, to be assumed to have resulted from equally natural impulses. In order to discover whether such assumption stands upon a secure foundation, we must, instead of giving undue importance to individual words, rather take a wide view of the *general tendency* of the H.G. Mute-system within historic times. This, if I mistake not, points to quite a different conclusion. That there has been a considerable approximation of the H.G. to the L.G. Mute-system during such period, is a fact of the utmost importance in relation to our discussion; but its importance lies in

[1] G. G., p. 149.

[2] See § 8 *antè*, especially the *notes*; see also § 54.

this, that that approximation was gradual and continuous from the very first appearance of the H.G. dialects in literature up to the comparatively recent time when those dialects began to assume the unity and fixity of a mature language. As to any previous phonetic movements, there is, of course, a total absence of external evidence. This being the case, one would have thought that sound reason would have led men to infer that that phonetic movement extended uniformly backwards into remoter ages, and not that it was at once reversed—unless, indeed, we are to act on the dangerous principle already abjured (§ 8 (*e*)), that what takes place when history is silent is just the opposite of that which takes place when history speaks.

(*d*) To make clear what has just been urged, let us expand Σ_y and Σ_z into the two series of individual consonants which they symbolize, appending the corresponding forms assumed at later epochs by the H.G. Mutes, thus :—

Σ_y		*k*	*h*	*g*	..	*t*	*th*	*d*	..	*p*	*f*	*b*
[Σ_z (ideal)		*ch*	*g*	*k*	..	*ts,z*	*d*	*t*	..	*ph*	*b*	*p*]
O.H.G. (9th cent.)		*ch*	*H*	*k*	..	*z*	*d*	*t*	..	*ph*	*F,V*	*p*
M.H.G.	(init.)	*K*	*H*	*G*	..	*z*	*d*	*t*	..	*pf*	*F,V*	*B*
„	(med.)	*ch*	*H*	*G*	..	*z*	*d*	*t*	..	*f*	*V*	*B*
„	(final)	*ch*	*CH*	*C*	..	*z*	*d*	*t*	..	*f*	*F*	*p*
N.H.G.	(init.)	*K*	*H*	*G*	..	*z*	*d*	*t*	..	*pf*	*F,V*	*B*
„	(med.)	*ch*	*H*	*G*	..	*z,ss*	*d*	*t*	..	*f*	*F*	*B*
„	(final)	*ch*	*H*	*G*	..	*z,sz*	*d*	*t*	..	*f*	*F*	*B*

This table[1], if I mistake not, tells a very distinct tale. Omitting the dental series for the present, we see that the latest H.G. forms agree with the L.G. to the extent of two thirds or six ninths as regards the labials, and, as regards the gutturals, to the extent of seven ninths; but going backwards in time to the ninth century, the degree of assimilation is reduced to one third or three ninths in each of those two families. As, then, we

[1] It is really Grimm's (Gesch., pp. 424–426). The ideally correct Σ_z is introduced for comparison. The capitals in the lower groups indicate points of assimilation to Σ_y.

have no evidence of any contrary movement immediately previous to the ninth century, the obvious inference is that, if we could trace the dialect still further and further back in time, even this amount of approximation to Σ_y would at last disappear, and we should arrive at the ideally perfect Σ_x.

(*e*) This coincidence of H.G. with L.G. in respect of *h* and *f*, when interpreted to mean incomplete evolution of Σ_x from Σ_y, is a serious stumbling-block in the way of some forms of the Chronological Hypothesis. Its antagonism to the views of Curtius has been already pointed out (§ 16 (*c*)); and its bearing on those of Mr. Sweet will be considered in Appendix D. I may remark in passing, however, that it is equally inconsistent with the *direct* evolution of Σ_x from Σ_y by Cross Compensation (§§ 18–22). For if, in both the guttural and labial family, one member (the Aspirate) was never altered, then, in a case of evolution of the kind just mentioned, the other two members should have been simply interchanged; that is, to the

should have answered	L.G.	*k*	*g*	and	*p*	*b*
		⋮	⋮		⋮	⋮
	H.G.	*g*	*k*	and	*b*	*p*

whereas we actually find

answered to by	L.G.	*k*	*g*	and	*p*	*b*
		⋮	⋮		⋮	⋮
	H.G.	*ch*	*k*	and	*ph*	*p*

Now why should or how could Σ_x, which duly answered to *g* by *k*, answer to *k* by *ch*? And if it already possessed the L.G. aspirates *h* and *f*, why should it go out of its way to create an incongruity by evolving a second set of aspirates? The incongruity becomes still more incongruous by comparison with the exact regularity of the dental family, in which the third member occupies its proper place, and which therefore is ideally perfect with reference to the dental family of Σ_y. Add to all this the remarkable fact that the purest extant O.H.G.[1] must thus have spontaneously left itself without

[1] "Strengalthochdeutsch" or "Alamannisch" (Schleicher, "Die D. Spr.," p. 97).

either *g* or *b*[1]. It will hardly be urged that the old *h* and *f* were on their way towards these mediæ. For, at the outset, the possibility of such a transition, as a matter of natural sound-change, may be safely denied; and, again, if they were on the way, why did they not proceed therein, as, by hypothesis, the dental aspirate must be represented as doing? The only satisfactory solution of all the difficulties seems to be, that at a remoter period both the guttural and labial family, just like the dental family, were completely differentiated at the same time, and in precisely the same way, as the corresponding families of Σ_x and Σ_y, and consequently that the appearance of L.G. mutes in H.G. is the result of a later partially-executed *assimilation* of the latter to the former,—in other words, that the gradual assimilation of Σ_z to Σ_y which is traceable during comparatively recent times is but a continuation of a movement which must have set in much earlier.

54.—(*a*) This proposition, in its relation to historical evidence, requires perhaps a little further discussion; for it has very important bearings in many directions. Let us begin by summing up the leading facts or phenomena which we have already met with in connexion with the subject. These are, I think:—(1) The existence of (apparently) H.G. forms at the beginning of our era (§ 52); (2) The appearance of L.G. and H.G. forms side by side, both then and centuries later, in the same localities (§ 53 (*b*)); (3) The non-appearance (*i. e.*, so far as our scanty knowledge reaches) of any H.G. tribes in history in the earliest part of our era;—(4) The progressive, but at length arrested, assimilation, within recent times, of the H.G. to the L.G. Mute-system.

(*b*) Now, with all these facts the hypothesis of this treatise is in strict harmony, and they with it. As to the first, it is clear that if Σ_z was evolved simultaneously with Σ_x and Σ_y out of a common central mute of each family many centuries, perhaps many chiliads of years before Christ, and was found existing many centuries after Christ, it must have existed at

[1] Schleicher, "Die D. Spr.," p. 100.

all intermediate periods. The discovery that H.G. words existed during such periods is therefore just what may be expected whenever any fresh sources of information become available.

(c) As to the second, it is a fundamental condition of our hypothesis that the dialects in which a functional phonetic relationship is mutually and simultaneously evolved should be actually in presence of one another and even commingled; and the phonetic equipoise, once established, may be maintained as long as that commixture remains at or about the same degree of intimacy. It may happen, as I have more than once remarked, that the dialects, after the evolution of their reciprocally adjusted Mute-systems, may not only tend to separation, but actually separate; and this was the case as regards the Cl. on the one side and the German, both High and Low, on the other. Or, on the contrary, social and political causes may check the tendency towards separation of the peoples using such dialects, and may have the effect of reducing or tending to reduce linguistic diversities to unity; and this seems to have been the case as regards the High and Low Germans with respect to each other. The incipient divergence between these two peoples indicated in the figure to § 46 (c) was carried no further; but in their migrations westward the one tribe or cluster of tribes threw in their lot with the other; and the resulting commixture ultimately became too intimate for the preservation of the phonetic equipoise originally established. I would suggest, in fact, that Σ_y and Σ_z, within and even before our era, were the characteristics, not so much of distinct tribes, as of different strata, so to say, of one and the same tribe or group of tribes: just as the Cockney dialect (§§ 38–41) and the polite English dialect are (or were) spoken by different, but overlapping and even commingled, strata of the inhabitants of one and the same town. Nay, further, as polite English is to the dialect just named, and as the Cl. dialect was both to the L.G. and to the H.G. (§ 44 (a)), so, I am inclined to think (in contradiction to their designations), was the L.G. to the H.G., the *Low* German being the language of the *higher* stratum of the people, and the *High* German of the lower. The

further divergence of the dialects having been thus arrested, the linguistic difference between the different strata of people became, for the most part, merely phonetic,—and phonetic, for the most part, only so far as the mutes were concerned. And this difference is perfectly consistent both with phonetic agreement in the other sets of vocables,—liquids, semivowels, vowels; and still more with that close relationship of grammar and vocabulary which must, early in our era, have characterized all the German tribes. I am not suggesting (for it would be going beyond all probability) that a H.G. substratum extended with and among *all* the L.G. tribes. Many of these were probably almost or quite free from such admixture. But there are others to which it may be more especially attributed; and such, perhaps, were the Goths and the nearly related, if not identical, Daci. For close as is the grammatical relationship between the German dialects in general, that between H.G. and Gothic is the closest of all[1]. Hence it comes to pass that in the list of Dacian words from Dioscorides, the number of those which have apparently a H.G. form is so comparatively large. These would probably be the popular names of plants current among the lower people.

(*d*) The most important bearing of the last subsection is upon the third and fourth of the facts in subsection (*a*). As to the third, it may now be conjectured why the H.G. dialect has left no evidence of its existence in those far-off ages. Its footing, if we are right, would be that of an obscure *patois* widely prevalent, perhaps, but among the humblest classes; and its phonetic peculiarities, if noticed at all, would be despised as vulgarisms[2]. The *consonant*-system of the language used by kings, nobles, priests, and warriors would be that of the L.G. dialects; and the names of the leading men, tribes, towns, and districts, which have been preserved for us

[1] Helfenstein: C. G., p. 7, and throughout the book.

[2] The old-world character even of N.H.G. harmonizes well with such a view of its former position. "German is, in many respects, much more archaic than Middle English, and may be said to stand to it in almost the same relation as Old English [Anglo-Saxon] does."—Mr. H. Sweet, "Hist. of Engl. Sounds," p. 40.

by the Romans, naturally exhibit the same phonetic characteristics. How then came Σ_z to the front? The cause may easily be found in the wars and still more in the great migrations of the different German tribes during the earlier centuries of our era[1]. England, Denmark, Scandinavia, France, Italy, and Spain were all more or less completely occupied by German tribes, whose original home was nearer the centre of Europe. Both war and migrations (which at the outset were really but military expeditions) would thus draw off, never to return, the very classes[2] among whom L.G. had its stronghold. The effect, at least in southern and south-eastern Germany, must have been nothing less than a social revolution: and in those tribes which originally contained a considerable H.G. substratum, the corresponding dialect would forthwith acquire a decided preponderance[3].

[1] "So viel steht fest, dass alle germanischen Dialekte einen sehr starken Ortswechsel....in jener Epoche erfahren haben, als die germanische Welt in ihren Vernichtungskampf mit der römischen eingetreten war, und die Völkerwanderung die deutschen Stämme von dem einen Ende Europas bis zum andern führte. Ganze Stämme, darunter die einst so mächtigen und berühmten Goten und Vandalen, verloren ihre nationale Selbständigkeit und gingen in anderen Völkern unter; mit ihnen starben ihre Sprachen aus. Aus Bündnissen und Wanderungen, aus inneren Kämpfen und auswärtigen Eroberungen ging im Laufe der Zeit eine Reihe von Verschmelzungs- und Austilgungsvorgängen, von Trennungen, Vereinigungen und Fusionen hervor, aber ohne dass man sich dadurch der Einheit genähert hatte; und vor vierthalb Jahrhunderten, zu der Zeit als das jetzige Deutsch sich zuerst als die allgemeine Landessprache geltend zu machen anfing, herrschte in unserem Vaterlande noch ebendieselbe babylonische Sprachverwirrung wie um den Beginn der christlichen Zeitrechnung."—Whitney (Jolly), pp. 242, 243.

[2] Not all, of course, but perhaps the bulk of them; and, on the other hand, a considerable fraction of the H.G. substratum may have accompanied some of the migrations.

[3] May not some of the earlier movements among the Germans, of which we obtain an occasional glimpse in history, be reasonably attributed to the coexistence on the same soil, but with imperfect amalgamation, of such dialectically divided strata as the above remarks suppose? Take, for example, the well-known case of the *Batavi*, "pars Chattorum seditione domestica pulsi," as Tacitus says (Hist., iv. 12), who settled on the Lowlands about the mouth of the Rhine, where to this day the people are

(*e*) If now a subsequent amalgamation of the remaining people took place, what could ensue but the process represented by our fourth fact? To talk of a language or dialect (as some do of H.G.) setting out on a given line of phonetic development and, when at or near the end, turning round and, *proprio motu*, retracing its own path, may not be sheer nonsense; but it is certainly at variance with all that we know of unconstrained linguistic movement. A homogeneous language, undisturbed by external influence, should move on in one and the same direction, in accordance with the physical or metaphysical tendencies which have contributed to set it in motion; and when a language does not so move, we may always expect to find some source of perturbation external to it. If then in the 9th century the H.G. Mute-system approached much more nearly to the ideal Σ_x than it did in later times, and if in the 19th it exhibits a considerable divergence therefrom and an extensive assimilation to Σ_y, we may be absolutely certain that this phenomenon can have resulted from no other cause than the action of the dialect of which Σ_y was the characteristic upon the dialect of which Σ_x was the characteristic—in short, from a fusion of dialects previously distinct; and the equilibrium ultimately reached in N.H.G. is the result of a compromise between the two. In point of fact, we have here an admirable exemplification of the formulation of § 10 (*c*).

(*f*) The general character of this compromise is stamped upon the table in § 53 (*d*). In the case both of the Gutturals and Labials, the H.G. gave way. Its first concession in both families was clearly the adoption of the L.G. spirants in place of

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purely L.G.; while those (or the bulk of those) who remained behind were the ancestors of the H.G. Hessians of later times. So again, perhaps with respect to the rising that drove Ulfilas and his Gothic followers across the Danube. In this case, indeed, religion rather than race may have led to the tumult; but although history tells us absolutely nothing further about the persecutors, these certainly occupied the territory, and were doubtless the descendants, of the very Daci among whom (if I have understood Dioscorides aright) both H. and L.G. forms were current side by side.

its own mediæ. In the case of *h* for *g* it is difficult to determine whether any, and, if any, what, intermediate phonetic stage was required, or was possible. But the passage from *b* to *f* could hardly, by the Principle of Transition[1], have been immediate: for *b* is a stopped, *f* an unstopped, and therefore (in this respect) weaker, sound; on the other hand, *b* is a soft, *f* a hard, and therefore (in this respect) stronger, sound. Consequently the first stage in the passage must have been the unstopping of *b*—a debilitation, yielding the soft spirant *v*; and the subsequent raising of *v* to *f* was due, of course, not to natural development, but, as it were, to external violence—that is, to the assimilating influence of the commingled L.G. dialect. Here, I think, we have the explanation of a phenomenon which, although it has hitherto received but scanty attention, is really very remarkable: I allude to the frequent appearance of the labial *v* in H.G. in the place and *now* with the power of *f*,—its appearance, moreover, *always*, I believe, where the ideal Σ_z should exhibit *b* (corresponding to *f* of Σ_y), and *never* where Σ_z exhibits its native spirant in correspondence to the *p* of Σ_y. These *v*'s, I would suggest, originally represented older *b*'s which, when H.G. became a written language, and indeed at a much later period (for, as far as the written language goes, they are really more frequent in M.H.G. than in O.H.G.), had only advanced (at least *apud plebem*) halfway towards the *f* of the assimilating dialect; they had become unstopped, but not yet hard or voiceless. The symbol, thus fixed, remained unaltered; but the sound, as often happens, left the sign behind, and proceeded to complete assimilation with the sound of *f*. If now the commingled Low Germans had held firm to their *k* and *p*, the labial and guttural families of the fused dialects might have been completely assimilated to the L.G. type: but in these points the latter dialect went over to the H.G. The L.G. dental spirant, however, seems then to have offered (as it still

[1] "Le Principe de Transition consiste en ce que la permutation ne marche que *pas à pas* et ne fait qu'*un pas à la fois*." (Baudry: G.C., p. 82.) It is therefore identical with the famous Linnean doctrine "*Natura non facit saltus*," so widely recognised in the natural sciences.

offers) considerable difficulty to those unpractised in it[1]. Here, accordingly, the H.G., holding fast to its media, preserved the relative phonetic position of the whole dental family; and the L.G. had again to adjust itself to the H.G.[2]

(*g*) Upon the whole, the preponderance of the H.G. is, in the oldest remains of the dialect, very decidedly marked. One may easily imagine, however, that, long after the great migratory movements before referred to, L.G. would maintain its *prestige* as the old polite and literary dialect, and that, before the final fusion of any two related mutes (or, rather, in the present case, absorption of one by the other), duplicate forms exhibiting both would remain in use side by side. Such commixture is abundant in M.H.G.[3], and indeed remains to the present time. It may not now appear in literature; for a primary requirement of written language is orthographic uniformity. But if we turn to the spoken language, we find that extensive areas in North Germany are still occupied by various *Platt-Deutsch* dialects; while the purest H.G. is almost confined to the south and south-east. And, again, where the written language exhibits assimilation to Low German forms, the pronunciation often refuses to follow. Thus, we write *raub, sand, sammlung*, &c.; but in pronouncing such words we finish, in each case, with a distinct tenuis[4]; that is to say, although these final letters have assumed L.G. forms to the eye, the pronunciation remains pretty nearly the same as if the ideally correct H.G. consonants (*p, t, k,*) were actually written[5].

(*h*) Thus does the evidence which is adduced to prove in-

[1] See Appendix B, last note.

[2] It is worth noting that nearly all the supposed "unverschoben" forms quoted by Grimm (Gesch., p. 485) are dentals, indicating, as we might expect, a somewhat stubborn resistance on the part of L.G. to complete absorption into H.G.

[3] See Schleicher, "Die D. Spr.," p. 201.

[4] See Note 1 to § 35.

[5] Max Müller employs this discrepancy (Lectures, ii. 226) as an illustration of the way in which he supposes the Goths to have "fixed the second series, the *g, d, b*'s, in their national utterance, as *k, t, p*." But for everything there is a reason.

complete *dissimilation* of Σ_x from Σ_y (which are assumed to have been previously *identical*) join with various other considerations in proving rather the incomplete *assimilation* of one of these systems to the other,—the two having previously been totally *distinct*. And now, in concluding this part of my subject, I may be permitted to repeat that Reflex Dissimilation is a very different process from a simple phonetic *Verschiebung*. It does not set in, run its course, produce given effects within a given (and comparatively limited) time, and then cease. As has already been often stated, when once the Dissimilating Sentiment has been, by whatever means, evoked, it may remain vigorous for an indefinite period of time. The length of that period is determined by a set of circumstances, political and social, totally independent of language. But as long as two strata of people (to take the simplest case) mutually maintain that relative position, or degree of contact or commixture, which renders correlative phonetic variation possible, so long will this incessant translation and retranslation of correlated sounds, when once established, continue. What happens if that relationship is afterwards destroyed has likewise been pointed out, but may be again stated:—If one stratum be separated from the other (as was the Cl. from the N. European), each will take with it its own phonetic system, and the Dissimilating Sentiment will gradually or rapidly die out, according as the separation is gradual or instantaneous, and as intercourse is or is not thereafter for a time kept up. But if one stratum be absorbed or welded into the other, either its system will completely disappear in the system of the other, if that other is socially or numerically far superior; or, if there be some approach to social and numerical equality, the result will be a phonetic compromise, such as is exhibited by the H.G. of to-day.

(*i*) Even while such absorption is going on, dissimilation may continue active, although in fewer and fewer cases, up to the very moment of complete amalgamation: and as long as it *is* active, it seizes and alters any suitable word presented to it by the commingled dialect, without inquiring whence that word may have come. Thus, formerly, members of the London

"residuum", on acquiring, from any source, a new word commencing with *v*, would at once, as a matter of course, transform that *v* into *w*. For the Dissimilating Sentiment operates with reference only to the standard dialect in its presence, and takes no note (how should it?) whether the words which call it into play are native to that dialect or imported from abroad. And some of the examples quoted by Grimm seem to illustrate a precisely similar mode of action on the part of the Old High Germans. Thus the words *curt-us* and *moneta* were borrowed from the Latin at an early date[1]. But the mass of the High Germans no longer knew anything of the cognate dialects of Italy. The reaction of H.G. against the Cl. dialects, which would at one time have turned *curt* into *gurd*—and *moneta* into *monida*, had ceased for many a century. Not so, however, the reaction of the popular (H.G.) dialect against the polite and literary; which, if not still L.G., inherited, in the popular ear, its *prestige*. This process, though dying, was not yet dead; and on the appearance of foreign words in the latter dialect the former seized and treated them as if they were really L.G. forms, producing, in the above examples, *kurz* and *muniza*. In this case, indeed, the popular dialect, over a certain area, ultimately absorbed the standard one; so that these and such-like forms came to the surface; nevertheless *kurt* survived in literature to a comparatively late stage of the language.

55.—(*a*) And here I shall desist from further treatment of the main body of my subject. Of the imperfections of the foregoing sketch no one can be more sensible than myself. But I am consoled by a confidence that the principles laid down in it, whatever may become of my manner of applying them, will furnish a safe and satisfactory foundation for a correct theory of Grimm's Law. Of one thing, at any rate, I am quite sure: —that problems of this kind are not to be solved by shutting one's self up in one's study, and dreaming of "Heldenzeiten" and the like fallacious explanations of one aspect of the puzzle,

[1] Förstemann, however (Gesch., i. 370), following Lottner (Z-S., xi. 186), connects *kurz* directly with the A-S. *scort* (our *short*).

which only make the other aspects more perplexing than before. For we have here to deal not so much with material as with movement, not so much with fossils as with life; we have not to reconstruct an earlier language, but to account for a given set of changes. Our business, therefore, is to observe, as well as to theorize—to observe, not only what is petrified in written remains, but what is going on in the living spoken languages and dialects about us; and although we may never discover another example of the remarkable triplicate relationship which characterizes our Σ_x, Σ_y, and Σ_z, and which probably arose from a combination of circumstances unlikely to recur, I have no doubt that many examples of the simpler case of dual relationship will be detected; some of which may tend, still more directly than those I have collected, towards elucidating Grimm's Law. Or, conversely, other linguistic problems may be met with, towards the solution of which some or other of the principles laid down in this book may be made to contribute. I shall conclude my little treatise by noticing a problem which seems to me to offer a case in point.

(*b*) Among the minor philological questions recently discussed on the Continent is one relating to the nature of the primitive I-E. *k*-sound. The leading facts involved in the question are these:—The Aryan languages employ not only a hard or pure *k*, but also, in many words, a *k* which, by a known affection, has degenerated almost into a sibilant (ç). Let us call this the Aryan or Sibilant affection. Now, in the corresponding words of *most* of the European languages, the representative of this gutturo-sibilant is *invariably* a *pure k*. On the other hand, some European languages, especially Greek and Latin, exhibit another affection of *k*, which, in its earliest known stage, resembles a combination of *k* with the labial semivowel *w*. Let us call this the European or Labial affection. In Latin it appears in the form *qu*—the guttural still maintaining its preponderance; but in Greek (as in some old Italian and Keltic dialects) the labial semivowel seems to have gradually acquired the supremacy—first becoming consonantized and then casting off the guttural altogether; so that (omitting dialectic forms, and a few cases of dentalization) we

have to suppose the series—*kw* (=κϝ), *kb*, *kp*, *p* (π). But here again, just as to the *Aryan* or *Sibilant* affection most of the *European* dialects answer with a pure *k*, so, in turn, to this *European* or *Labial* affection the old *Aryan* dialects answer with a pure *k*[1].

(*c*) This singular behaviour on the part of *k* might still perhaps have remained uninvestigated but for another remarkable fact. The Lithuanian and Slavonic, which are *European* dialects (once, and probably long, in contact with the old German dialects), exhibit in this matter of the *k*'s, a nearly complete agreement with the Aryan: that is, to the European or Labial affection they answer with a pure *k*; and where the other European dialects maintain pure *k*, they answer, in precisely the same cases as the Aryan (so far as the vocabularies agree), with a sibilant; thus:—

Skt.	O.Sl.	Li.	Lat.	Grk.
çata	=*suto*	=*szimtas*	=*centum*	=ἑκατόν:
çvan	=*su-ka*	=*sz̊u* (*szuns*)	=*canis*	=κυών:
naç-ati	=*nes-ti*	=*nèsz-ti*	=*nanciscor*	=ἔνεγκα:
praç-	=*prositi*	=*pirsz-ti*	=*precor*	=πράκ-jω:

which may suffice as examples of the Sibilant affection answering to the European pure *k*. On the other hand:—

Grk.	Lat.	Skt.	
πέπτω, πόπανον	=*coquo*, *popina*	=*pak'-*, *pak'ati*	=O.Sl. *peką*:
πέμπε (πέντε)	=*quinque*	=*pañk'an*	=Li. *penki*:
ἕπομαι	=*sequor*	=*sak'-*, *sak'ate*	=,, *sekù*:
ἡπαρτ-	=*jecur*	=*yakan*	=,, *jekna*:

which may suffice as examples of the Labial affection answering[2] to the Aryan pure *k*. It must be added, however, that a large number of *k*'s appear *both* in Europe and in Asia as pure *k*'s.

56.—(*a*) Before making any remarks of my own upon these peculiarities, I shall glance as rapidly as possible at two or

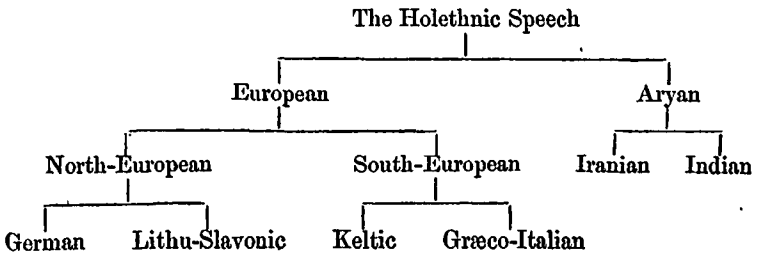
[1] For our present purpose the Aryan pure *k* must be assumed to include (as it once actually did) the Skt palatal *k*, generally represented, in our type, by *k'*, but sometimes by *c*.

[2] See the preceding note.

three of the leading attempts which have already been made both to elucidate them and to assign them their proper place in a general scheme of scientific Indo-European philology. It is a necessary preliminary to observe that their principal interest has hitherto been assumed to lie in their bearing upon what may be called the *genealogy* of the successively-diverging languages of the Indo-European family. With a view to precision of treatment, it has been found convenient by philologists to assign the languages, which thus successively branch off, to a series of linguistic "periods", during each of which the parent language of a subsequent group of languages is assumed to have existed in the form of a single and homogeneous language[1]. Such an assumption, if not pushed to the extreme of denying the existence of dialectic differences within the successive parent tongues, may probably in some cases be found to correspond with real historical periods, during which the tribes speaking those languages exhibited a general ethnic unity[2].

(*b*) But with the later "periods" we have in this place not much to do: our subject carries us back to the earliest of all,

[1] The following genealogical tree is based on Fick's in his I-G. W-B., p. 1051:—



I have substituted the assumed languages for the corresponding peoples, as given by Fick, and have omitted the later subdivisions. Here and elsewhere I confine the term Aryan to the Asiatic branch of the Holethnos—an application of the term which is, I believe, universal on the Continent. Its extension to include the Europeans seems to me to be justified by no sufficient reasons.

[2] But see on this point Max Müller, Lectures, i. 205, 206; who, however, if I understand him aright, runs to the opposite extreme of refusing to all such periods any historical reality whatever.

—to the *Urvolk* or Holethnos, with its primitive speech, and to the original Separation of the people into two sections, one of which remained in Asia, while the other moved off towards, and ultimately reached, Europe. In virtue of this ethnic separation there must, in course of time, have ensued (supposing each of the sections of people to have maintained a general unity) a corresponding linguistic division into a clearly marked European, and a clearly marked Asiatic (Aryan), "period". In point of fact the old European dialects on the one side, and the old Aryan dialects on the other, actually do exhibit grammatical and phonetic characteristics of their own. But the first application[1] of the phenomena exhibited by the primitive *k* was to the denial of any such clear and decisive original Separation as the "period" theory asserts, and to the bridging over, as it were, of the broad and deep gulf which was supposed to divide the one group of dialects from the other. For, it was urged, the Li-Sl. really agrees in some important points (as, *e. g.*, in the splitting or radiation of *a* into *a*, *e*, and *o*, and in the evolution of *l* from *r*) with the European division; but in its treatment of *k* it agrees just as completely with the Aryan division. If, therefore, on the former ground the Li-Sl. must be assigned to the one division, it must on the latter ground be just as decisively assigned to the other; that is to say, it belongs to both at once, and forms, as it were, a bond of union between them; so that we are no longer justified in imagining any such broadly-marked separation between the two as the "period" theory requires[2].

57.—(*a*) To such views as these an elaborate reply has been offered by Dr. Fick[3]; and so far as they are necessarily ini-

[1] By Dr. Joh. Schmidt: "Die Verwandtschaftsverhältnisse der I-G. Sprachen": Weimar, 1872.

[2] Bopp was not hampered by the rigid "periodism" of the younger Continental Philologists. The relationship of the Li-Sl. to the Aryan had attracted his attention; but he attributes it to the longer and later connexion between the two than between other European dialects and the Aryan.—See his V. G., p. 39; especially the note.

[3] "Die ehemalige Spracheinheit der Indogermanen Europas": Göttingen, 1873.

mical to a period of general linguistic agreement of all the European tribes, his reply must be considered altogether successful. But his treatment of the purely phonetic question is seriously affected by two antecedent considerations:—first, his determination to vindicate the "period" theory in its most uncompromising form, so that his phonetic hypothesis holds a place completely subordinate thereto; and, secondly, his assumption throughout that the guttural peculiarities in question, although they did not originate, were yet developed, subsequently to the original Separation. His fundamental proposition is that the Holethnic speech comprised two distinct *k*-sounds[1]. Let us designate these[2] by k_1 and k_2. Then k_1 is that *k* which is represented in Asia by *k* pure, and in Europe, except among the Lithu-Slaves, *sometimes* (and formerly, Fick thinks, always) by *kw*, &c.; and k_2 is that *k* which is represented in Asia by ç and in Europe (except as before) always by *k* pure.

(*b*) Consequently there requires explanation a complex series of phenomena, which may be tabulated thus:—

$$\begin{cases} k_1 \text{ appears in Europe } \textit{generally} \text{ as } k \text{ pure;} \\ k_1 \quad ,, \quad ,, \quad \textit{sometimes} \text{ as } kw = qu = \pi; \\ k_2 \quad ,, \quad ,, \quad \textit{always} \text{ as } k \text{ pure:} \end{cases}$$

$$\begin{cases} k_1 \text{ appears in Aryan } \textit{always} \text{ as } k \text{ pure}^3; \\ k_2 \quad ,, \quad ,, \quad ,, \quad \text{as } ç. \end{cases}$$

or reversely:—

$$\begin{cases} \text{European } k \text{ pure represents } k_2 \text{ in } \textit{all} \text{ cases and } k_1 \text{ in } \textit{most} \text{ cases;} \\ \quad ,, \quad kw \ (= qu, \&c.) \text{ represents } k_1 \text{ in the remaining cases.} \end{cases}$$

$$\begin{cases} \text{Aryan } k \text{ pure represents } k_1 \text{ in } \textit{all} \text{ cases;} \\ \quad ,, \quad ç \text{ represents } k_2 \text{ in } \textit{all} \text{ cases.} \end{cases}$$

[1] Ascoli, in Italy, was, I believe, the first to enunciate this view. He was followed by M. Havet in France ("Revue critique," Nov. 23, 1872). Fick concedes priority to both, although he appears to have conceived the idea independently.

[2] I borrow the convenient notation of M. Havet; that of Fick would require special type for one of the *k*'s; while his use of a bare *k* for the other is sometimes misleading.

[3] See note to § 55 (*b*).

This complexity is, as we have seen, further complicated by the circumstance that one old European dialect (the Li-Sl.) sides with the Aryan. But two important facts which immediately require notice are:—(1) that the characteristic affection of k_2 has disappeared in the other dialects of Europe; and (2) that a great majority of the *k*'s which, in these dialects, correspond to k_1, and should therefore on Fick's hypothesis exhibit the labial affection, actually exhibit no affection at all, but are, in fact, like the *k*'s representing k_2, *pure k*'s.

(*c*) To account for this, Fick invokes a summary process which he calls *Verwischung* or "Obliteration", in virtue whereof the Aryans in Asia and the Lithu-Slaves in Europe are supposed to have independently cleared away the Labial affection[1]; while the other Europeans are supposed to have similarly cleared away the Sibilant affection; so that k_1 in the former case, and k_2 in the latter, became pure *k*. These other Europeans, moreover (as was remarked in § 55 *ad fin.* and (*b*) above), must, on this hypothesis, have treated the majority of the labialized *k*'s in precisely the same way. But here, again, Fick is ready for us with a general *Verschmelzung* or "Fusion", whereby most of the descendants of k_1 and all the descendants of k_2 have in Europe become *verschmolzen* into a single sound, viz. the pure *k*.

58.—(*a*) Of all such summary modes of cutting away difficulties there are who harbour a not unnatural suspicion; for nothing is easier than to imagine that we understand or have accounted for a phenomenon or a process when we have merely learnt or invented a specious term which only conceals the fact that we know little or nothing about it. I should like, therefore, humbly to inquire what may be meant, for exam-

[1] If, at least, we are to attribute that affection to Holethnic times. Fick's opinion on this point, however, is obscure; indeed he seems to leave it at last for the reader to determine:—"Hierbei ist völlig gleichgültig *in welcher Periode* man sich diese Afficirung vollzogen denkt" (p. 28). I shall assume, for simplicity, that Fick's k_1, representing an incipient labial affection of *k* pure, was primitive; otherwise it will be necessary to suppose that the bulk of the Europeans introduced that affection merely to get rid of it again; which is absurd.

ple, by this *Verwischung*. And our first business, of course, when any such new or doubtful principle presents itself, is to see whether or not it harmonizes with old and well-established ones. Thus the weak points of the prevalent conjectural hypotheses of Grimm's Law became apparent when these were tested by the indubitable Principle of Least Effort. And in the case before us we may again use this very same principle as a touchstone. For if (adopting the line of argument employed in Appendix B in reference to the Aspirates) we consider the ultimate forms assumed by k_1 and k_2, we can hardly arrive at any other conclusion than that both of these, in comparison with k pure, must originally have exhibited at least an incipient corruption or debilitation. For the ultimate form, or at least one ultimate form, of k_1 is p, and the ultimate form of k_2 is $ç$: both p and $ç$ (the latter, of course, much more than the former) are *weaker* sounds than k pure; and consequently the primitive affection whose development gave rise to those weaker sounds can hardly have been anything else than an incipient debilitation. In the case of k_2, whose characteristic affection was clearly the more virulent, and whose history is simpler than that of k_1, we observe that the debilitation was, on Fick's hypothesis, developed both by the Aryans in Asia, and (independently) by the Lithu-Slaves in Europe; while all traces of it were "obliterated" among the bulk of the Europeans.

(*b*) Here therefore Fick's *Verwischung* means that all the Europeans, except the Lithu-Slaves, on no limited scale, and for no apparent reason, raised a weaker sound to a stronger. As to k_1, which is represented to have made up the remainder of the series of k's, Fick leaves us on several points in considerable doubt. If we are to treat it as having been characterized by the labial affection from all time, then the Holethnos must have been totally destitute of a pure k,—which is hardly credible (§§ 24, 25, *antè*); then, too, the Aryans in Asia and the Lithu-Slaves in Europe (in spite of the adverse influence of the other Europeans) must have independently changed this affected k to pure k, just as these other Europeans are supposed to have changed k_2. In addition to this we have the further

remarkable fact that the other Europeans also represent k_1 in the great majority of instances by pure k: so that if k_1 was affected from all time, or at least in Holethnic times (as I think Fick intends us to suppose), then the bulk of the Europeans must have followed two incongruous modes of treatment of k_1, by one of which it became k pure, while by the other it became *qu* and π or p[1].

(*c*) The incongruities involved in these phonetic movements are such as one must hesitate to believe in. For k_1 and k_2 are supposed to have had each its own domain in the Holethnic vocabulary, and therefore in the vocabularies of *all* the constituent dialects at the Separation. And though (§§ 33–36 *antè* and *notes*) all the dialects, when separated, need not have followed precisely the same lines of development, yet their several lines could hardly have run in opposite directions, or their final forms be inconsistent with one another, especially among those dialects which in other respects evince the closest reciprocal affinity. Nevertheless, on Fick's hypothesis, we have to attribute such diversity and inconsistency twice over to groups of dialects thus nearly related; *i. e.*, to the South-European as opposed to the Aryan, and the German as opposed to the Lithu-Slavonic,—the one group in each case invariably sending k_2 up to k pure (or, in German, its equivalent), the other as invariably sending it down to a sibilant—the one in some cases maintaining *kw* (or its equivalent) sometimes lowering it to *p*, sometimes raising it to k pure, the other invariably raising it to k pure. These results are not merely diverse; they are mostly irreconcilable. Yet the antagonism between the more nearly allied dialects is perhaps even less wonderful than, on Fick's hypothesis, the independent identity of action on the part of the Aryan in Asia and the Lithu-Slavonic in Europe.

[1] "The old *qu*" in Latin is represented as "in full retreat" before the the simple $c=k$ (Spracheinheit, p. 13); and again, *kw*, both in Latin and Greek, as a "receding and continuously vanishing sound" (p. 21). But for a flatly contradictory view, see Corssen, "Aussprache", &c., i. 70:—"Dass auch innerhalb des Lateinischen, und zwar in der älteren und in der klassischen Zeit der Sprache, *qu* sich aus *c* entwickelt hat, zeigen die Wortformen 'o*qu*oltod' 'huju*sque*' (=*ce*)"—and others. Compare i. 356, *note*.

(*d*) With a view to obtain a definite conception of the extent of the supposed phonetic movements, at least in Europe, I have selected what appear to be primary words[1] involving k_1 and k_2, to the number of about 230, from Fick's second Abhandlung[2]. Dividing these proportionally, I find that about 30 per cent. involve k_2 and exhibit a sibilant either in Lithuanian or in Slavonic, or both. These among the rest of the Europeans were raised to pure *k*'s. Of the remaining 70 per cent., which should involve k_1, nearly all[3] exhibit pure *k* in Li-Sl., but in the rest of Europe they have to be divided into two sets or series: in the one set, comprising only about one fifth of them (or about 14 per cent of all the *k*'s) the labial affection is actually traceable; but in the other set, comprising no less than four fifths of them (or about 56 per cent. of all the *k*'s) such affection nowhere appears. Thus, by the hypothesis in debate, the bulk of the Europeans seem to have sent but a very small number of these *k*'s on to or towards the labial terminus; while they sent the vast majority in a totally different direction,—viz., towards *k* pure.

(*e*) The main objections to Fick's hypothesis therefore seem to me to be these:—that it would leave the Holethnic speech without a pure *k*; that it would doubly offend against received methods of philological reasoning,—first, by *refusing* to the Holethnos a *pure* sound which is *common* to both of its great divisions, and, secondly, by *assigning* to it two *affections* of that sound, which we only know as *special* and *characteristic*, one of the European and the other of the Aryan division; that it would attribute to the most closely related dialects irreconcilably diverse modes of treating the same sounds, and, on the other hand, identical modes to dialects more remotely related—these identical modes being supposed to exhibit themselves quite independently of each

[1] The *proportional* values which follow will remain very nearly the same if all the *secondary* words be likewise included.

[2] "Das Vorkommen von *k*, und k_2 im Wortschatze der europäischen Spracheinheit."

[3] The two or three exceptions scarcely affect the case. They are probably due to contact with the other Europeans.

other and under circumstances of a widely differing character; that the assignment of two *k*'s to the Holethnos would only account, in reality, for these identical modes of treatment; and that the suggestions intended to account for the diverse modes of treatment in the more nearly related dialects involve, firstly, incongruous developments of one and the same sound (k_1) in one and the same set of dialects, and, secondly, in the case of both *k*'s, a general disobedience to the Principle of Least Effort, in which disobedience, indeed, the Aryan and Li-Sl. dialects concur, so far as k_1 is concerned. To meet the last-cited objection, it should be shown either that, if *k* pure is to be derived from k_1 and k_2, or either of them, these sounds (one or both) are stronger than *k* pure, or else, on the other hand, if *k* pure is stronger than either or both of them, that they are (one or both) derived from *k* pure.

59.—(*a*) This alternative has been boldly met by M. Havet, who has recently returned to a consideration of this question[1]. To k_1 and k_2 he gives distinct and known phonetic values. He considers k_1 to have been originally = our *kw* or the Latin *qu*—exactly, always, and everywhere—and to have been *stronger* than *k* pure; which, indeed, he represents as derivable from the former by natural debilitation. K_2, again, was originally nothing else than *k* pure itself; and from it the sibilated guttural is represented as having descended. Such, it is said, were the values of k_1 and k_2 in the Holethnic speech; and although the various descendent dialects may have modified those values, they have all, in one way or another, preserved the distinction between the two *k*'s.

(*b*) By the provision that k_1 is essentially stronger than *k* pure is meant that *kw* (*qu*) may (and does) descend to *k* pure by natural debilitation, and that, "in spite of current opinions, the contrary change cannot take place." Hence (to omit finer degrees of debilitation) *kw*, *k* pure, *k'* (or *c*),

[1] "L'unité européenne, et la question des deux *k* Ario-européens": in the *Mémoires de la Société de Linguistique*, ii. 4, 1874; an article brimful of condensed facts and reasonings.

and *ç* are successive stages of phonetic *descent*. At the Separation each of the two great sections of the Holethnos took with it the old *kw* and *k*. Among the Aryans the original k_1 (*kw*) descended to *k* pure, and the original k_2 (*k* pure) to *k'* (*c*) and ultimately to *ç*. The bulk of the Europeans, on the contrary, maintained k_2 (*k* pure) as it was, and brought down k_1 (*kw*) in most cases to the same level; but the Lithu-Slaves, by a merely "fortuitous coincidence", reduced k_1 in all cases to *k* pure, and k_2 (=*k* pure) in all cases to a sibilant, in precisely the same way, and to precisely the same extent, as the Aryans, so far as the two vocabularies correspond. To this "coincidence", and to some other minor features of the hypothesis, objection might fairly be taken. But I pass on to the corner-stone of the whole, viz., the relative phonetic value attributed to the two *k*'s, or, at least, the value given to k_1.

60.—(*a*) The merit of this hypothesis consists in the respect it appears to pay to the Principle of Least Effort. Whether or not that respect is real as well as apparent depends on the correctness of Havet's determination of the relative value of *kw* and *k* pure. On this point I confess I do not feel that certainty which M. Havet's precision of language is intended to inspire. We have already seen (§ 58 (*a*)) that *kw* (*qu*) in several dialects ultimately degenerates into *p*. This phonetic characteristic (implying that the parent sound had set out on a given line of debilitation) is in accordance with the physiological characteristic of *kw* as indicated by its vocal formation. For in pronouncing this combination the point of contact of the tongue with the palate is much forwarder in the vocal passage than the point of contact at which the purest *k* (*k* before *a*=*ah*, for example) must be produced; and this is the physical characteristic of a weaker sound (§ 6). Authority on this point may not count for much, or else that of Bopp[1], Curtius[2], and Corssen[3] might be quoted as in direct opposition to the views of Havet: in fact, their

[1] V. G., § 84, 1. [2] Grk Et., p. 451. [3] "Aussprache," &c., i. 70.

opinions are, no doubt, the "current opinions" which Havet expressly controverts. Nor, perhaps, is the point in dispute to be decided by the analogy supplied by the combination (*ky*) of the palatal semivowel (*y*) with *k*, which is universally admitted to be an initial debilitation, and the physical formation of which differs from that of *kw* almost entirely in the absence of protrusion of the lips.

(*b*) But neither, on the other hand, does the evidence adduced by M. Havet in favour of his own view seem to me to be decisive; for he bases his doctrine respecting the relationship of k_1 (*kw*) to *k* pure upon examples taken from the Romance languages as compared with the Latin. Now, in accordance with the principle laid down in § 28 (*d*) *antè*, § 35 *note* 1, *et alibi*, and worked out in relation to the Aspirates in § 33, this circumstance may very seriously weaken, if not entirely destroy, the foundations of his doctrine. For in the Romance languages we meet with Latin sounds, not after they have pursued a uniform line of natural development or decline among one and the same people, but after they have been subjected to the mispronunciation of foreign and semi-barbarous races, to whom some of them would probably be strange and difficult and who would therefore modify them to suit the previous education or want of education of their own vocal organs.

(*c*) In France, especially, whence Havet draws most of his examples, it is perfectly well known that there was considerable difficulty with one element in the combination *kw* (*qu*), viz. the labial semivowel. The uncombined semivowel in Latin, indeed, had probably passed into the spirant *v* before the extensive spread of that language over western Europe. But the old semivowel (*w*) was at a later period introduced over the same area, as well as into Italy, by invading Germans (§ 53 (*d*)). The natives, however, seem then to have been unable to pronounce it alone; and their attempts to do so led to the combination therewith of the guttural *g*[1]. And the physiological peculiarity which led, at the outset, to this

[1] For some examples, see Max Müller, Lectures, ii. 295–297. The process is ust the reverse of what took place in the oldest Latin; where the semivowel overpowered and at last annihilated the conjoined mediæ and as-

accretion, also led subsequently to the complete preponderance of the new guttural and to the disappearance of the old semivowel,—often to the eye, and nearly always to the ear[1]. But no one, I presume, would argue from this irregularity, or phonetic contortion, that *w* represents a naturally stronger sound than *g*, and that the sound of *g* is the legitimate descendant of the other. Now, turning to the closely related combination *kw* (*qu*), we observe, firstly, that it already existed, and was imposed on the Gauls and others with the language in which it was indigenous, or at least naturalized (See next section); and, secondly, that the guttural thus provided for them in advance was more powerful than the *g* with which they provided themselves. We may fairly argue therefore, *à fortiori*, that, if a guttural could be inserted where none before existed, and, when inserted (although thus inorganic, and not the strongest sound of its family), could overpower the conjoined semivowel, much rather might it be expected that *k*, the strongest of all mutes, already in combination with *w*, would, *among the same people*, decisively overpower and annihilate the same semivowel.

(*d*) It seems to me therefore altogether unsafe to base upon the history of *kw* (*qu*) in any of the Romance languages the doctrine that that combination is naturally and essentially stronger than *k* pure, and that the latter is always and everywhere the legitimate descendant of the former. The case is rather one of those in which the natural operation of the Principle of Least Effort has been forcibly set aside by the physical peculiarities of a people on whom a foreign language has been imposed from without. If it is not an exception that proves our great rule, it is, at any rate, one that does not invalidate it. Admirable therefore as is M. Havet's article in

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piratæ; as, *e. g.*, in *vivere* (=*wiwere*) for *gwi-gw*-ere; *suāvis* for *suadwis* *levis* for *leghwis*, &c. In spite of the caution suggested by these and such like cases, Benfey, in his Shorter Grammar, is inclined to lay down the general rule that, where the combinations *kw*, *gw*, &c., occur in the older I.-E. dialects, the semivowel alone was the primitive element, and the conjoined mute a later accretion.

[1] For many familiar examples, see Brachet's "Dictionnaire Historique. s. v. "*Gâcher*."

many points, it is, I think, infirm in just that one on which, above all, the value of most of his other opinions depends; in a word, he attributes to natural development a change due rather to what may be called the violent action of external forces. It is as if we should argue that the old aspirate *th* (=*θ*, *þ*) is stronger than, and the natural parent of, the tenuis *t*, on the ground that the former is, in the Romance languages, represented by the latter—or even that our soft spirants *v* and *th* (=*ð*, *dh*) hold a similar relation to *b* and *d*, because the negroes of America say *bery*, *brudder*, *dis*, &c., for *very*, *brother*, *this*, &c. M. Havet's illustrations are indeed of great value, but in support of a conclusion of a very different kind; for if the combination *kw* made its appearance among any of the older Indo-Europeans, and if it was afterwards in any cases replaced by *k* pure, this similarity of treatment implies similarity of history; and one might infer that the combination was no more native to those people than to the Gallic races of our own era.

(*e*) On the whole, then, M. Havet's view of the relative strength of *kw* and *k* pure seems to me to be inconsistent both with the comparative physiology of the two sounds, with the analogy supplied by the relationship between *ky* and *k*, and with the tendency of *kw* to become *p*; while the examples by whose aid he endeavours to prove the natural derivation of the pure from the affected *k* seem to me to prove something altogether diverse. Apart from these points, his hypothesis differs very little from that of Fick, and has little or no advantage over it; so that what has already been urged against the one (§ 58 (*e*)) applies pretty nearly as much against the other. M. Havet does, indeed, appear to endow the Holethnos with a pure *k* in his k_2; but as he represents the *k* pure of later times to be the first stage of descent from *kw* (except where the latter branches off towards *p*), and as we can hardly suppose both a stronger sound and a weaker and immediately derivable sound to have coexisted from the birth of language, it appears to be an almost necessary extension of his hypothesis that the same mode of phonetic generation characterized still earlier times; so that, going further

and further back, we shall at last reach a period when *kw* alone, and not *k* pure, existed[1].

61.—(*a*) It will have been seen from the preceding sections (55–60, and the *notes*) that scarcely any philological problem can have given occasion to more diverse and hostile opinions than that involving the genealogy and relationship of the various I-E. *k*-sounds. If to those opinions I here venture to add one more, it is because I have grounds for thinking that some of the principles enunciated in the former part of this treatise will enable me to attack the problem upon a totally different side from any towards which previous inquirers have advanced. I must remark at the outset that the ethnic question, involving the relationship of the Lithu-Slaves to the Aryans on the one side and to the rest of the Europeans on the other, is distinct from, and as regards our present inquiry subordinate to, the linguistic question, and not this to that. If, indeed, we had trustworthy records of the primitive history of that section of the Europeans, we should no doubt be able to account for all the linguistic phenomena which now appear so perplexing. As things stand, however, the only means we have of obtaining, conversely, a glimpse or two of the history of the people is furnished by language; and hence I shall for a time leave the ethnic question altogether out of sight.

(*b*) Referring now to §§ 18–22, 28 (*d*), and especially to §§ 38–43, we can hardly fail, I think, to be struck by the great similarity between the phenomena presented by the *distribution* of the various I-E. *k*-sounds and those presented by the cases there discussed and formulated; for the most striking feature of this distribution is the diagonal or cross relationship between the pure and impure *k*'s of one set of I-E. dialects and those of the other set; by which, of course, I mean that

[1] Grassmann, in Kuhn's Z-S., vol. ix. ("Ueber die Verbindung der stummen Konsonanten mit folgendem *V*") had already laid down the doctrine that, wherever such a combination as *kw* can be traced in any of the I-E. dialects, it was in every case the primitive and parent sound, whatever variations might represent it in the related dialects.—The older conflicting opinions on this perplexing *Verbindung* are admirably summarized by Baudry, G. C., pp. 114, 115.

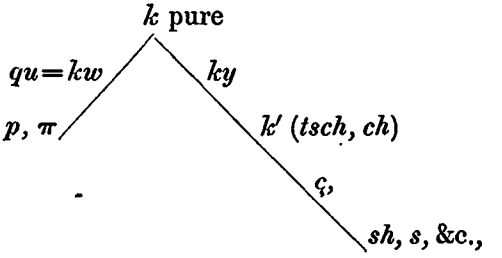
to the European pure *k* on the one side answers the Aryan and Li-Sl. impure *k* on the other; while to the European impure *k* (so far as it prevails) answers the Aryan and Li-Sl. pure *k*. To explain this has clearly been the chief difficulty of previous hypotheses. Nevertheless the general phenomenon appears to me to be referable to a class which will, by this time, be quite familiar to the attentive reader; and it may be analysed into a number of particular and constituent phenomena, which will, I hope, be equally familiar. In the maintenance of the pure *k*, for example, in opposition to the impure ones, we recognise phonetic Resistance on both sides; while the respective debilitations or corruptions seem almost as clearly to exhibit phonetic Retreat (§ 42). But these correlative processes necessarily imply that, while they were in action, the dialects exhibiting them were in presence of each other (§ 41 (*a*)): hence the movements among the *k*'s must be thrown far back into Holethnic times, when the (subsequent) European and the (subsequent) Aryan groups of dialects stood to each other in the relation of single dialects in contact.

(*c*) Again, of the three sounds in debate (viz. *k* pure and the two affected *k*'s), the first alone is common to, or has the same phonetic value in, both sets of dialects: hence, on the hypothesis of this book, we must suppose either that, before the setting-in of Dissimilation (that is, the Mutual Resistance and Retreat just mentioned), the Holethnos possessed two impure *k*'s, but no pure *k*, and that the evolution of a pure *k* out of one impure *k* by one section of the Holethnos led to the evolution of an exactly equivalent pure *k* out of the other impure *k* by the other section: all which, when followed out into detail, appears to me unlikely and even absurd;—or else that the entire Holethnos once possessed but a single and uniform *k*, which was no other than the purest and strongest *k*; and that a dialectic fissure, as it were, was originated by the debilitation, on the part of one section of the people, of a number of these pure *k*'s by an incipient sibilant or labial affection, as the case might be; and that the other section, while preserving those *k*'s pure,

nevertheless proceeded ultimately to respond to that debilitation by an incipient labial or sibilant affection of some of the pure *k*'s which were not attacked in the first-mentioned section: all which constitutes a case precisely similar to those already treated of in former parts of this book. And, once more, we have to observe that the two impure *k*'s differ phonetically from each other. This implies that the pure and impure *k* of one set of dialects are not the result of interchange, by Cross Compensation, between the impure and pure *k* of the other set (§ 22 (*c*)), but that, if those *k*'s are to be correlated at all, it must be by way of Reflex and Mutual Dissimilation (§§ 28 (*d*), 33, 40 (*b*), &c.); for this process does not necessarily require that the mutually counterbalancing impure sounds derived from the stronger parent sound should be exact phonetic equivalents of each other. I propose therefore to maintain that *k* pure must have been (as by its nature it ought to have been) the original single parent sound from which the impure *k*'s were derived—one by ordinary sound-weakening, and the other by Reflex Dissimilation.

(*d*) But a question that may here fairly arise is, whether the Aryan and the European debilitations did not differ too widely to allow of their correlation. If, indeed, we conside only the ultimate forms (*p* and ç) assumed by the one and the other, we shall of course answer at once that they did. But *p* and ç are really the extremities, as it were, of divergent phonetic radii. Remounting from those extremities we find that these radii converge towards, if they do not actually meet at, a common phonetic centre, which is nothing else than *k* pure; so that the phonetic distance between the nearest known points to *k* pure is vastly less than that between the extremities. In the case of the radius extending to ç we may consider the centre as actually reached; that is to say, we can, out of the facts of language, establish a direct line of descent from *k* pure to varieties of the sibilant. In truth, scarcely any line of phonetic debilitation is better known in our family of languages than this. And in every case the first step downwards is traceable to the accretion after *k* pure of the palatal semivowel *y* (=German *j*) or to the combination with

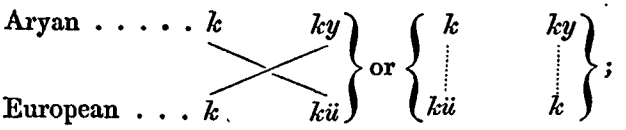
k of the palatal vowel *i* (=Engl. *ee*)—the further steps being represented by *k'* (=*tsch*=Engl. *ch* in *church*, nearly), by *ç*=Germ. *ch* in *ich* nearly, and by *s'* (=Engl. *sh*); from which the passage to *s* is not uncommon; and that to *z*, and even to complete disappearance, is perfectly possible. The initial movement down the other radius should, by analogy, be caused by the accretion after *k* pure of the labial semivowel *w*, or of some closely-allied "parasitic" sound (to borrow Curtius's picturesque term) which might readily pass into *w*. In this case the two phonetic radii might be represented to the eye as follows:—



This order of evolution for *kw* is, however, as we have seen, reversed by M. Havet; and it must be admitted that the negative evidence arising out of the difficulty of establishing our assumed order from facts supplied by the younger languages seems to be in his favour. But it is really quite as much in my own; and my remarks in § 60 (*d*) are intended to hint a reason why, even though *kw* (*qu*) should sometimes appear, in the ancient, as well as in the Romance languages, to have been supplanted by *k* pure, the former may nevertheless represent a primitive corruption of the latter. For if we suppose the evolution of *kw* out of *k* pure to be due to the action of Reflex Dissimilation, we can hardly look upon the former as an indigenous, spontaneous, and unconstrained debilitation of the latter. It would, in that case, be due to the instigation of the Dissimilating Sentiment, whose vigour in inciting to phonetic movements which would not otherwise have taken place we have already had ample opportunities of observing (§§ 18–22, 38, 39, *et alibi*). It might therefore (§ 28 (*d*)) differ in value from the debilitation which evoked it, pretty much as

the reproduction of a strange and difficult sound by foreigners differs from the native value of that sound; and its relationship to *k* pure would differ from the relationship of the same original debilitation to *k* pure pretty nearly as a forced contortion differs from a spontaneous weakening.

(*e*) All this virtually assumes (what I shall presently show to be supported by facts) that the Aryan, Palatal, or Sibilant affection supplied the initial movement which incited to Reflex Dissimilation on the part of the commingled dialects. Still (to return to the point from which the preceding subsection started), if the earliest form of the European or Labial affection is necessarily to be taken as *kw* (*qu*), the difference between it and *ky* may appear to be somewhat too wide to allow us, with perfect satisfaction, to correlate the sounds in the way suggested. Considering, however, the fluctuating value of the closely-related labial vowel *u*,—in Greek, for example, on the one side, and French on the other, as compared with Latin,—we shall not be transgressing probability if we conjecture that the Labial affection may originally have had a value intermediate to *k* and *kw*, such as may be represented by *kü*. This, of course, is a considerable approximation to *ky*, but is justified by the Principle of Transition (§ 54 (*f*), *note*), on the ground that the phonetic distance, as it were, from *k* pure to *kw* is too wide to be covered by a single even though forced leap. By the intermediation of *kü*, at any rate, the European affection is brought sufficiently near to the Aryan to allow us readily to believe that the former was intended to be a reproduction, by Reflex Dissimilation, of the latter[1]. Reducing the whole movement, therefore, under the formulations of § 18 or of § 28 (*c*), we now obtain the correlative values—



[1] The intermediate combination (*kü*) might perhaps almost equally well be assigned to the Aryan side, and *kw* be left as the original value of the European affection. But see the next note.

which formulation correctly summarizes nearly all the phenomena of the distribution of the several *k*-sounds; while the European reproduction of the initial Aryan debilitation is supposed to differ slightly in phonetic value from the latter, just as (*a*) and (a) in § 44 (*f*) differ from **A**[1].

62—(*a*) I have already forestalled the inquiry as to the dialectic place of origin of the whole movement. This has been assigned to the conjoined Aryan and Lithu-Slavonic dialect, but solely, so far, on the ground that the sibilation of *k* has the aspect of a spontaneous and indigenous debilitation for which we can account, while the character of the

[1] I was originally inclined to trace the Aryan debilitation, as well as the European, to an earlier *kü*, intermediate to it and *k* pure. In this way the primitive values of both the impure *k*'s would be equalized, and to the oldest Aryan *k*-system (*k* pure and *kü*) would diagonally correspond, sound for sound, the oldest European *k*-system (*kü* and *k* pure). This intermediate value to *k* and *ky* might be defended by the frequent inflexional and derivational passage of *u* to *i* in our family of languages, which implies a passage through the intermediate value *ü*. But as my theory does not really require such equalization of the primitive corruptions, I have thought it best, following what evidence we have in respect to *ky*, to make this a direct debilitation of *k* pure; and only to employ the intermediate *kü* on one side (*i. e.*, between *k* and *kw*) instead of both.—I learn from a short note in M. Havet's article just now examined, that Ascoli has already suggested that the primitive I-E. *k* had (in all cases?) a value which he represents by k^y (where *y*, I suppose, is = my *ü*), and which gave off a k^i in Asia and a k^u in Europe. I may therefore perhaps be permitted to explain that these sections of mine on the *k*-sounds (except so much as relates to M. Havet's views) were originally written immediately after the appearance of Fick's book, and before any other part of my treatise, and, further, that I have been unfortunately prevented by a series of accidents from reading anything that the learned Italian has written on the subject. That the general idea therefore of referring the impure *k*'s to a common and intermediate sound should have been independently conceived is *really* a "fortuitous coincidence." The value of the idea, however, depends on the use made of it. As regards this point, I am still unaware of the way in which Ascoli proposes to explain that cross relationship which, as I have above remarked, constitutes the main difficulty of the problem,—that is to say, why that k^y which was labialized in Europe became pure *k* in Asia, while that k^y which was sibilated in Asia became pure *k* in Europe.

European affection is just the opposite. But more may be said for the same view. For, following the line of argument adopted in § 32 with reference to the Aspirates, we observe, in the first place, that the action of this palatal debilitation is, among the Aryans, uniform and regular; and, in the second place, that it is virulent and extensive. In the Aryan Vocabulary of Fick's I-G. Wörterbuch, no less than some 40 per cent. of the primitive words originally involving *k* pure exhibit the semi-sibilant (ç); while *in Skt* about 15 per cent. more exhibit the palatal (*k'* or *c*). These *k*'s or *c*'s, indeed, although they answer to the *k* of Fick or k_1 of Havet, may really be considered to represent both a continued action of the Aryan impetus towards this form of debilitation, and also an arrestation of the debilitation before it had produced its full effect.

(*b*) In contrast, on the other hand, with the virulence and wide extent of the native Aryan debilitation stands the mildness and narrow extent of the reflex (European) debilitation; and in contrast with the uniformity of the former stands the diversity observable in the latter. No one dialect seems to have hit upon the correct value. In Greek, the parasitic element assumed, at an early date, the power of a consonant, at least so far as the assimilation of the conjoined consonant is concerned. Hence for the labialized *k* we generally find π. In this treatment of the *k* some of the old Italian dialects resembled the Greek: so did the Gallo-British; while the other leading Keltic dialect (the Old Irish) appears to have reduced all its *k*'s to a uniform value generally represented by *c*. The Latin differed from the other Italian dialects in preserving the rigid combination *qu*, with a decided guttural preponderance. And, the normal change from *kw* (*qu*) to *hw* being made, the German dialects exhibit an original affinity with the Latin. These varieties of treatment I would attribute, as I attributed the varieties of Aspiration (§ 33) to more or less (but not completely) successful efforts, on the part of some dialects, to acquire a sound indigenous only to a commingled dialect, but new and strange, and in effect foreign and difficult to the first-named dialects. And so,

substituting for forcible imposition of a sound an attempted imitation instigated by the Dissimilating Sentiment, we may actually discover, in the early history of the European affection, a close resemblance to the way in which the Latin *qu* made its appearance in the Romance languages (§ 60 (*c*)). It would therefore not be surprising. if the later history of the older combination should also be found to exhibit, in some cases, a resemblance to the later history of its Latin descendant and representative.

(*c*) The inciting debilitation having originated on the Aryan side, the reflex movement on the European side would naturally set in later and advance more slowly. This may help to explain why the labial affection is demonstrably traceable in so small a proportion of the European *k*'s; but this is not all the explanation. On the ground of the statistics quoted in § 58 (*d*), I should conjecture that the mutual adjustment of the *k*'s between the two sets of dialects was cut short by some overruling movement, social or linguistic, pretty nearly at the stage at which we find it[1]. If it had been completed, we should perhaps have found, judging from the distribution of the Aspirates (§ 14 (*c*)), some 60 or 70 per cent. of sibilated *k*'s in Asia corresponding to pretty nearly the same percentage of pure *k*'s in Europe; and 30 or 40 per cent. of labialized *k*'s in Europe corresponding to pretty nearly the same percentage of pure *k*'s in Asia. Now there are two known movements to which such an arrestation of the dissimilating process may be attributed. One is the original Separation of the Holethnos into the European and Asiatic sections; the other (on the theory of this treatise) is the rise and spread of the greater phonetic movement formulated by Grimm's Law. The former, by removing each of the two great dialects in which

[1] I would not deny that some small number more of the European *k*'s may have been partially labialized,—or, more properly speaking, that the pure *k* may in some words have coexisted for a time, and have been used indifferently, with a labialized *k*, and that, when the Dissimilating Sentiment was destroyed, the pure *k* may have reasserted its rights, and have driven out the other. But I see no way of admitting that all the *k*'s which were not sibilated in Asia were originally labialized in Europe and that from four fifths of them the labialism was afterwards expelled.

action and reaction were going on beyond the influence of the other, would almost immediately destroy the Dissimilating Sentiment in the European division (see §§ 37 (*b*), 54 (*a*), *et alibi*). The latter would produce a similar effect by occupying more rapidly and vigorously the same ground as the minor movement (and much besides); and, so far as phonetic change was subservient to linguistic extension (§§ 28–30), by rendering the operations of the more limited movement unnecessary. It may be thought daring and rash to propose to settle the relative chronology of such almost primeval movements; but as I have, throughout this treatise, maintained that the greater and more complete *Verschiebung* (so-called) must have altogether preceded the Separation; so I shall in the next section try to show some reason for thinking that the smaller and less complete interchange, affecting *k* only, must have preceded the greater. And (unless the cause is quite unknown) it is to the overwhelming influence of this greater movement that I would attribute the extinction of the smaller while yet incomplete.

(*d*) The ethnic question, deferred from § 61 (*a*), will now require but little remark. If we are justified in tracing the distribution of the *k*-sounds to a mutual understanding, as it were, between (for our purpose) two dialects, we necessarily assume what has already been so often insisted on (viz., the contact of those dialects with each other) as the fundamental and indispensable condition of the symmetrical dissimilation between them. We have no option then but to maintain that, at the epoch when the changes of the *k* were in progress, the Li-Sl. section of the Holethnos was in pretty close cohesion with the Aryan section, partook with it of a common debilitation, and acted and reacted with it upon the Europeans proper. This is perfectly consistent with the general consonantal system of the Li-Sl. dialect, by means of which I have endeavoured to adjust the relative geographical position of the tribe; it only suggests further that the Aryan tribes must have occupied that part of the Cl. district which extended to and overlapped the Li-Sl. district (see the figure, § 46 (*b*)). The difficulty therefore to be explained with respect to the

Lithu-Slaves is not, as it seems to me, why, although they were subsequently a division of the North Europeans, their phonetic system should be what it is, but, conversely, why, their phonetic affinities with the Classical and especially the Aryan sections of the Holethnos being so close, they should subsequently have drifted away among the North Europeans.

(*e*) In accordance with the views I have now endeavoured to propound, the phonetic movements among the *k*'s, so far as they went, may be laid out in terms precisely similar to those employed in § 40 respecting another movement:—(1) There is originally a single language (the Holethnic) employing a single sound of a certain character (*k*); (2) this language divides, or tends to divide, into (for our present purpose) two dialects, an Asiatic and a European; (3) in one of these (the Asiatic) a debilitation (*ky*) of that sound springs up and spreads; (4) the other dialect (the European) at first resists that debilitation; but (5) the two dialects continue in presence of each other; hence (6), by the habit of answering to *ky* by *k* pure a perception of incongruity and the Dissimilating Sentiment are at last awakened among the Europeans; and (7), under the influence of the former, this people proceed to adjust (as they suppose) their sounds to those of the commingled dialect; but, diverted by the latter, their efforts only result in a counterbalancing corruption of such of their own pure *k*'s as correspond to the unaffected Asiatic *k*'s,—the sound they actually produce, however, not being an exact reproduction of the Asiatic *ky*, but differing from it in being a stage nearer to *kw* (say *kü*), from which it ultimately descended or advanced to *kw* (*qu*).

(*f*) This whole movement, therefore, seems to me to be reducible to precisely the same general principles as those to which the phenomena of Grimm's Law itself are referable. And our examination both of the one and the other proves, I think, that all the principal phonetic characteristics of the I-E. languages were impressed upon them in their common infancy and adolescence, and not in their independent and fixed maturity. Not only is this conclusion just what the abstract nature of things requires, but it is also one which allows the

amount of phonetic change in both cases to be greatly reduced and to be fairly distributed. Further it permits us to view the Holethnic speech under the same aspect as we view any other great language—*i. e.*, as a conglomerate of dialects (§§ 35 (*b*), 44 (*h*)) agreeing with and differing from one another in every possible variety of combination. If therefore any peculiar phonetic movement set-in in one dialect, the behaviour of the rest in reference to that movement would be determined partly by the inherent genius of each, and partly by the existing relationship of each towards that one. The sides taken by some of the leading dialects in respect of these changes of *k* are indeed instructively different from those taken by them in respect of the greater (and later?) movement. Thus, in respect of the former, the Li-Sl. sides completely with the Aryan, and the Græco-Italian and the combined Germans are banded against those dialects; while in respect of the latter, the Græco-Italian coheres with the Aryan, and the Li-Sl. exhibits, in one point, an apparent assimilation to L.G. This behaviour of the Græco-Italian is worthy of observation; for it clearly marks out the dialect as once occupying the front rank among the dialects of greatest resistance (§ 44 (*a*)). For when the Aryan admitted a headlong debilitation, the Græco-Italian was found on the same (resisting) side as the Germans, and, like these, only admitted the much less virulent and much more limited results of Reflex Dissimilation; but when the German dialects gave way to still more sweeping corruptions, the Graeco-Italian passed over to the resisting camp with the Aryan, and again only admitted (and to a less extent than the Aryan,—§14 (*c*)) the results of a like Dissimilation.

63.—(*a*) Let us now return to the question whether the affections of the *k* may not have belonged to a remoter epoch than the phonetic movement represented by Grimm's Law. I have suggested that they did; for the former movement, no less than the latter, probably represented an attempt to extend the powers of language by Phonetic Variation (§§ 28–30). But the latter movement was not only much more extensive

than the former; it was also more extensive than was required (§ 27 (*b*)). Hence, if the latter be supposed to have come first in time, there will seem to be left no reason why the less extensive movement (supposing it had set in at all) should have been taken up by other dialects than that in which it originated; whereas, supposing the smaller to have come first, it might, although thus taken up, still prove quite insufficient for the requirements of a rapidly growing language, and might be superseded and rendered unnecessary by the greater movement. But as this mode of reasoning is necessarily inconclusive, I will venture to indicate a few facts which seem to me to furnish more cogent evidence to the same effect. The reader will remember that, by the theory of this book, the *k*'s we have been treating of (*i. e.* the affected *k*'s of both species, and the pure *k*'s to which I have endeavoured to restore them) although commonly spoken of as the "Primitive" *k*'s, are in reality only the "Classical" *k*'s,—the guttural Tenues of Σ_x alone. The *k*'s of Σ_y, however, and the *k*'s of Σ_z have been shown (§§ 24–26) to be equally primitive with the *k*'s of Σ_x; all the three sets of *k* contributing, in proportion to their number, towards the one unbroken series of primeval Tenues out of which the other mutes were afterwards evolved. Hence, if any phonetic movement affected the guttural Tenuis in the early era which preceded the greater so-called *Verschiebung*, that movement cannot be supposed to have selected as its victims just those *k*'s (represented in L.G. by *h*), and just those only, which were subsequently to constitute the share of the Cl. tribes. It should also have similarly affected (i) those *k*'s (let us represent them by **k**) which subsequently formed the share of the L.G. tribes, and which were represented by the Cl. *g* and the O.H.G. *ch*; and (ii) those *k*'s (let us represent them by **ĸ**) which subsequently formed the share of the H.G. tribes, and which were represented by a Cl. χ (*gh*, *h*) and a L.G. *g*. Further, these several sections of the original unbroken series of *k*'s should have exhibited, in proportion to their extent, the same mutually dissimilating action as is exhibited by the (later) Cl. section; that is to say, just as we found (§ 61 (*e*)) the

answered to by the

Aryan	*k*	*ky*
European	*kü* (*kw*) . .	*k*

so we ought to find (i) the

answered to by the

Aryan	**k**	**ky**
European	**kü**(**kw**) . .	**k**

and likewise (ii) the

answered to by the

Aryan	ɬ	ɬ*y*
European	ɬ*ü* (ɬ*w*) . .	ɬ

so that, after the completion of the more extensive debilitations whereby **k** became the Cl. *g* and H.G. *ch*, and ɬ the Cl. χ (*gh*, *h*) and L.G. *g*, we ought, in virtue of (i), to find traces of—

(1) A section of labialized *k*'s in L.G. and of labialized *g*'s in Græco-Italian, on the one hand, corresponding to a section of pure *g*'s in the Aryan and Li-Sl. dialects on the other (*i. e.*, L.G. *kw* (*qu*)=Lat. *gw* (*v*)=Grk γ^F (*β*)=Aryan and Li-Sl. *g* pure);

(2) A section of pure *k*'s in L.G. and of pure *g*'s in Græco-Italian, on the one hand, corresponding to a section of affected *g*'s in the Aryan and Li-Sl. on the other (*i. e.*, L.G. *k*=Lat. *g* =Grk *γ*=Aryan *gy* (*j*)=Li-Sl. *z*).

And, in virtue of (ii), we ought to find traces of—

(3) A section of labialized *k*'s in O.H.G. (=labialized *g*'s in L.G.) and of labialized *χ*'s (*h*'s) in Græco-Italian, on the one hand, corresponding to a section of pure Aspirates in Aryan and pure Mediæ in Li-Sl. on the other (*i. e.*, H.G. *kw* (*qu*) = L.G. *gw* = Lat. *hw*, *gw* (*v*) = Grk χ^F (*ϕ*) = Aryan *gh* = Li-Sl. *g*);

(4) A section of pure *k*'s in O.H.G. (=pure *g*'s in L.G.) and of pure *χ*'s (*h*'s) in Græco-Italian, on the one hand, corresponding to a section of affected Aryan Aspirates and Li-Sl. Mediæ on the other (*i. e.*, H.G. *k*=L.G. *g*=Græco-Italian *χ*, *h*, *g*=Aryan *ghy* (*h*?)=Li-Sl. *z*).

Besides these two pairs of sections there should also be, in each case, both (i) and (ii), a considerable section of *k*'s not

yet, or not finally, reached by the Dissimilating Process (§ 62 (*c*)); so that both Aryans and Europeans would, as regards such sections, agree in exhibiting pure gutturals. But leaving aside these sections, from which reflex action was absent, and which therefore do not much concern us here, we have to observe, with reference to the affected and mutually dissimilated sections, that, although we ought to be able to *trace* all the relationships just detailed, yet, on the supposition of the priority of the movement among the *k*'s to that represented by Grimm's Law, we should be prepared for a considerable disturbance, by the later and much more thorough movement, of the original regularity of the correspondence in the Cl. dialects between the pure and affected k's and k's, such as still characterises the affected and unaffected Cl. *k*'s, properly so called, which remained undisturbed. Indeed a too great regularity in the former case would actually be prejudicial to the proposed chronology of the two phonetic movements; for while it might be held to imply the total absence of any such later and disturbing movement, it would also be perfectly consistent with the hypothesis of a simultaneous affection of the Cl. guttural Tenues, Mediæ, and Aspiratæ, at a period *subsequent* to the evolution of the latter two sets of consonants from the other set.

(*b*) As, then, if the affections of the *k* preceded the other and more extensive movement, we ought to meet with the phenomena just detailed, so, conversely, if we actually meet with those phenomena, we may, in default of any other or better explanation thereof, treat them as evidence that our chronological adjustment of the two movements is correct. But in proceeding to demonstrate the four cases of relationship specified in the preceding subsection, I must express my regret that I am unable either to fall back on any researches of other inquirers, or myself to devote any great amount of time and labour to the collecting of evidence. Nevertheless, considering the supplementary nature of these later sections of my treatise, I may perhaps claim to have done enough, if I shall have adduced a sufficient number of examples to place the existence of such relationships beyond dispute. Follow-

ing then the order already adopted, we ought to find instances in which—

(1) L.G. *kw* (*qu*) = Lat. *gw*(*v*) = Grk γϝ(β), in opposition to an Aryan and Li-Sl. *g* pure. Accordingly:

O.N. *kwân-* (*kon-*), Goth. *qên-*, O.S. *quân-*, A-S. *cwên-*, Engl. *queen* (and *quean*?) = γυν-ή and Bœot. βαν-ά (for γϝαν-*a*); against Skt *gnâ*[1], Zend *ghen-a*[1], (but also Skt *jân-i*, Zend *jén-i*[2],) Pruss. *gann-a* (but O.Sl. *žen-a*).

O.N. *kwal-*, O.S. *qual-*, A-S. *cwal-*, O.H.G. *quāl-* and *chwāl-*, akin to O.H.G. *quell-an*, Engl. *quell* (and *kill*?[3]) (= βαλ-?); against Skt *gal-*, or Lith. *gél-u*[4].

O.N. *kwe-ð-a*, Goth. *qi-þ-an* (*qaþ*), A-S. *cwe-ð-an*, Engl. *quo-th*, *be-quea-th* (root *kwa-*); against Skt *gâ-* "sing," *ga-d* "speak" (but Lith. *žad-*).

O.N. *kwam-* (*kom-*[5]), Goth. *qim-* (*qam-*), O.S. *kum-an*, A-S. *cum-an*[5], Engl. *come*[5], O.H.G. *quëm-an*, N. *komm-en*[5], = Lat. *ven-ire* (for *gwen-* or *gwem-ire*) = Grk βαίν-ω (*i. e.* βαν-jω for γϝαμ-jω); against Skt, Zend, and O. Pers. *gam-*.

O.N. *kwerk-* = O.H.G. *quërc-a* and *querech-ela* "gullet" (but Lat. *gurgula* and Grk γέργερος[6] are apparently irregular); against the Skt *gargara*, "whirlpool, gulf,"—no doubt a duplication of *gar-*, "swallow," which is correctly (on my

[1] Fick, W-B., p. 57. I must here express my obligation to this boldly conceived work, without which the labour of collecting and collating even the few examples I cite would have been greatly increased.

[2] Id., p. 257. See the first example (*cennan*, &c.) in subsection (*d*) *infrà*.

[3] Is the annihilation of the labial semivowel in *kill* due to Norman influence (§ 60 (*c*) *antè*)? The form *kill* cannot be traced far back in time; and, besides, we natives have rather an affection for the combination *qu*, just as our Saxon ancestors had for its equivalent *cw*.

[4] It is not quite clear to me that the Skt root *gal-*, "fall," (or as a causative "make fall") and the Li-Sl. *gal-*, "to hurt, to pain," are connected. According to Fick they ought to be; for (W-B., pp. 61, 518) he makes each equivalent to the German *kwal-*.

[5] The *u* and *o* represent a fusion of the semivowel with the conjoined vowel (See next section).

[6] This word, perhaps, in Græco-Italian, simulated onomatopœia; and hence the gutturals escaped affection, and appear identical with the Skt gutturals.

hypothesis) answered to by Lat. *vor-are* (for *gwor-are*) and Grk *βορ-* (*γϝορ-*), the root of *βι-βρώ-σκω*, &c.

O.N. *kwörn*, Go. *qairn-u*, A-S. *cwéorn*, Engl. *quern*, O.H.G. *quirn* and *chwirn-a*; against Lith. *girn-a* (but O.Sl. *žrun-y*).

The foregoing examples, together with that in the next subsection, leave no doubt, I think, as to the original correspondence, so far as they reach, of a European affected *k* to an Aryan pure *k* in that section of the *k*'s which afterwards became the Cl. *g*'s. If the number of such examples is small, so also is the proportion of Tenues in the L.G. Mute-system (§ 14 (*c*)) compared with their proportion in the Cl. system.

(*c*) One very important and interesting example, properly belonging to the foregoing series, deserves a special examination. This starts on the European side from the root *kwiw-* or *kwik-*, "live", *quiwa-* or *quika-* "living"; O.N. *kwikr*, Goth. *qiwa-*, O.S. *quik*, A-S. *cwic*, Engl. *quick*, O.H.G. *quëc* and *chèch*, = Lat. root *viv-* (for *gwiw-*) in *viv-us* (*gwiw-us*) and *viv-ere* (*gwiw-ere*) = Grk root *βιϝ-* (for *γϝιϝ-*) in *βίο-ς* (*i. e. βιϝο-ς*), &c. Here the labialism of the initial consonant is most decisively marked; and we should expect to find the purity of the initial *g* just as decisively preserved on the other side. So it is in the Lith. (*gyva-s*) and Pruss. (*giwa-s*); but the O.Sl. is *živu*, and all the Aryan derivatives are based on the forms *jīv-* and *jīva-*. It is manifest, therefore, that the last two dialects have gone astray from regularity; but they have done so in a way which still comports with my hypothesis. For the more violent and extensive phonetic movement so often referred to, in evolving the Aryan and Li-Sl. Mediæ from the primordial Tenues, would probably effect a complete arrestation of the older and but partially gratified Dissimilating process. It might well happen, therefore, that the original distinction between the *g*'s which once answered to the impure European *k*, and those which did not, should in sundry instances be forgotten, and that (at any rate after the Separation) the debilitation to which the latter were liable should in some odd cases attack the former also[1].

[1] That is to say: Immediately before the arrestation of the less by the

As to the final consonant of the same root (*gwiw-*, *giv-*), I am afraid this must remain, for the present, a *crux* for etymologists. M. Havet's remarks on the root, though not new in substance, are neatly put, and appear at first sight to explain the whole matter. The stem *jiv-* (he observes) of the Skt present tense (*jiv-ati*=*vivit*) proves the existence of a primitive *g* before the *i* and of a *w* after it; while the Latin *vixit* (*i. e. wig-sit*) shows a *w* before the *i* and a *g* after it; hence he constructs the form *gwigw-*, a supposed duplication of a still older *gwi-*, as the parent of the cluster of words above cited. Perhaps, in the absence of any suspicion of the existence and operation of such a process as Reflex Dissimilation, no other conclusion was open to him. But relying on this process, we may deny that the Labial affection is in any case Aryan, and that either the initial *j* or the final *v* (*w*) of *jiv-* has come from *gw-*. And the same, of course, is true of the Li-Sl. dialects, whose evidence is, in this particular example, singularly copious and clear. The original initial of the root therefore appears to have been *g* (=**k**) pure; and this was preserved on the Lith. side. But as to the terminal sound, although the combination **kw** (*gw*) was by our hypothesis peculiarly and distinctively European, yet no trace of its guttural element

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greater movement, the complete series of Aryan *k*'s would be divisible into: (1) palatized *k*'s answering to European pure *k*'s; (2) pure *k*'s answering to European labialized *k*'s; (3) pure *k*'s common to both dialects. Subsequently therefore to the greater movement, the resulting series of *g*'s would be similarly divisible into: (i) palatized *g*'s answering to European pure *g*'s; (ii) pure *g*'s answering to European labialized *g*'s; (iii) pure *g*'s common to both dialects. Now, although, as long as the European and Aryan dialects were in continuous presence, group (ii) might have been maintained pure by Phonetic Resistance (§ 42), it is not less likely that the extinction of the Dissimilating Sentiment, in relation to those *g*'s, may have followed upon the stoppage of correlative debilitation. But however this may have been, it would surely be absurd to suppose that the Aryans would for ever continue to cherish and transmit a distinction between groups (ii) and (iii) after the removal of the European influence upon which the preservation of group (ii) as pure *g*'s depended. If therefore the indigenous Aryan tendency to palatization still operated in any degree—as it continued to do (§ 62 (*a*)) in the case of *k*,—it is difficult to see why group (ii) should be necessarily exempt from its action.

is to be found either in Gothic or in Greek. In the latter especially, considering its fondness for duplication, we have every reason to think that, if the assumed combination had ever been formed, it would have been preserved, and that we should consequently have found the word *βίβος*, representing *γϝι-γϝος*, in preference to *βίος*, which properly represents *γϝι-ϝος*. So far the Aryan and European agree, and all is plain and easy. The difficulty arises from the Latin and several German dialects, which present us with a final guttural. To this guttural, however, in turn, no labialism can demonstrably be attributed; so that, where the guttural is, there is no labial; and where the labial is, there is not (and apparently could not have been) any guttural. It seems to me, therefore, that the twofold form of the final consonant has not yet been satisfactorily accounted for.

(*d*) Let us now proceed to the next case, comprising examples in which we should find

(2) L.G. *k* pure = Lat. *g* = Grk *γ*, in opposition to an Aryan and Li-Sl. affected *g*. Examples:

A-S. *cenn-an* "beget," *cyn* "kin," O.N. *kyn* = Lat. *gi-g(e)n-ere, gen-us* = Grk *γι-γ(ε)ν-ομαι, γέν-ος*: against Skt *jan-*, Zend *zan-*.[1]

O.N. *kenna-* and *knâ-*, Go. *kann-jan*, A-S. *cunn-an* and *cnaw-an*, O.H.G. *chenn-an, kenn-an*, and *chnâ-an*, Engl. *ken* and *know* = Lat. *(g)no-sco* = Grk *γι-γνώ-σκω*: against Skt *jna-*, Zend *zhnâ-*, Lith. *żin-*, Pruss. *sin-*, O.Sl. *znâ-*.

O.N. *knê*, Goth. *kniu*, O.H.G. *chniu*, O.S. *knio*, A-S. *cneow*, Engl. *knee* = Lat. *genu* = Grk *γόνυ*: against Skt *jânu, jnu*, Zend *zhnu*.

Goth. *kius-an*, O.N. *kjôs-a*, A-S. *ceós-an*, O.H.G. *chios-an* = Lat. *gus-tare* = Grk *γεύ-σομαι*: against Skt *jush-*, Zend *zus-*.

A-S., *ceow-an*, Engl. *chew*, O.H.G. *chiuw-an*, N.H.G. *kauen*; against O.Sl. *żwą, żu-jąti*.

O.N. *korn*, Goth. *kaurn-a*, O.H.G. *chorn*, A-S. and Engl.

[1] See the first example of subsection (*b*). The European noun (*kwân-*) seems to have been differentiated from the verb by labialism of the *k*, in accordance with § 28 *antè*.

corn = Lat. *grân-um*; against Lith. *zirn-i*, Pruss. *syrn-e*, O.Sl. *zrun-o*.

O.N. *akr*, Goth. *akr-s*, O.S. *akkar*, O.H.G. *achar*, Engl. *acre* = Lat. *ager* = Grk ἀγρό-ς · against Skt *ajra-*.

O.N. *ek*, Goth. *ik*, O.S. *ik*, O.H.G. *ich*, Engl. *I* = Lat. *eĝ-o*, = Grk ἐγ-ώ · against Lith. *asz*, O.Sl. *az-u*.

Goth. *rak-jan*, O.H.G. *recch-an*, N. *reck-en* = Lat. *reg-ere* = Grk ὀ-ρέγ-ω · against Skt and Zend *arj-*, Lith. *raž-*.

O.N. *björk*, A-S. *beorc*, Engl. *birch*, O.H.G. *pirch-a* and *birk-a* = Lat. *frax-inus* (*frag-s-*); against Skt *bhûrj-a*, Lith. *bérž-a-s*, O.Sl. *brěz-a*.

So far as these examples go, they leave as little doubt as the former series in reference to the reality of the phonetic reaction which they are cited to establish. It is, I presume, in consideration of such examples as those in both of the foregoing series, as well as from the analogy of the Cl. *k*'s, that Havet, following Ascoli, and in opposition to Fick, properly maintains, in the article just now examined, that there ought to have been (as his hypothesis requires him to put it) two primitive (*i. e.* Cl.) *g*'s. With the modification required by my own hypothesis, I go farther, and maintain not only that there *ought* to have been, or even that there *were*, but that there *must* have been two *affected* Cl. *g*'s, an Aryan and a European, together with a pure *g* on each side. One proviso should be added—namely, that those affected Aryan **k**'s which, in virtue of the greater so-called *Verschiebung*, fell in with the descending movement towards *g* could not, before that movement, have passed beyond the stage of *k'* (*c*)—perhaps even not beyond *ky*; for if they had reached, or nearly reached *ç*, they would already have been sibilated to a lower degree than *j*, and the only further degree of debilitation of which they would then have been susceptible would have been that into *z'* (*zh*), a sound which does not occur in the old Aryan phonetic system. This proviso, however, very well accords with the view (§ 62 (*c*)) that the dissimilation of the *k*'s was only partially executed when the greater movement set in.

(*e*) In the next place we ought to find examples in which

(3) H.G. *kw* = L.G. *gw* = Lat. *hw* (*v*), or, within words, *gw* (*v*) = Grk χ^F (ϕ); in opposition to an unaffected Aryan *gh* and Li-Sl. *g*.

Under this case, however, I know of only one example; and in that, the traces of the given relationship are partially obliterated among the Germans. The example is represented by our word "snow," A-S. *snâw*; where it would seem that the guttural of the combination *gw* has disappeared, as in Latin[1]. So too in the cognate dialects: Goth. *snaiw*, O.H.G. *snêw* or (in the verb) *snîw-*. But in Græco-Italian the combination is well preserved: Lat. *nix*, *niv-is* and *ningu-is* (for *snigw-is*), or (in the verb) *ningu-ere* and *niv-ere* (= *snigw-ere*), where *g* stands for the primitive Aspirate; while the Greek form of the root is *νιφ-* (*i.e.* *σνιχ^F-*), as in (*τὴν*) *νίφ-α*, *ἡ νίφάς*, *νιφει*, &c. On the other side, the root in Zend is *çnigh-*, in Lith. *snëg-*, O.Sl. *sněg-*, Pruss. *snaig-*: in all which the guttural is, as it should be, free from any affection.

(*f*) Lastly we come to case (4), in which H.G. *k* = L.G. *g* = Lat. *h* (*g*) = Grk χ, in opposition to an affected Aspirate in Aryan, and an affected Media in Li-Sl. Examples:

O.H.G. *car-ni*, or *gar-ni* = O.N. *gar-nir*, "bowel," akin to Grk *χολ-άδες*, and Lat. *hīr-a* (Plaut.) whence *hilla* (*i.e.* *hir-ula*) and *her-nia*; against Skt *hir-â*, and Lith. *żar-nà*.

O.H.G. *gî-ên*, N.H.G. *gäh-nen* = Lat. *hi-are*, &c. = Grk *χά-σκω*, *χαί-ν-ω* (*i.e.*, *χαν-yω*), &c.; against Skt *hâ-*, Zend *zâ-*, Lith. *żi-*, O.Sl. *zij-*.

O.H.G. *zunkâ* and *zunga*, O.N. *tunga*, Goth. *tuggôn-*, O.S. *tunga*, A-S. *tunge*, Engl. *tongue* = Lat. *lingua* (for *dingua*); against Skt *jih-vā*, Zend *hiz-va*, O.Pers. *izâva*, Lith. *lëżùw-is*, O.Sl. *języ-ku*. (But in the Li-Sl. dialects this word seems to have been confused with the following.)

H.G. *leck-en*, Goth. *bi-laig-on*, A-S. *licc-ian*, Engl. *lick* = Lat. *ling-o* = Grk *λείχ-ω*; against Skt *rih-* and *lih-*, Lith. *lëżiù*, O.Sl. *liżą*.

O.H.G. *wëk-an* and *wëg-an*, O.N. *weg-a*, Goth. *ga-wig-an*

[1] With us, *-ow* (as in morrow, borrow, bow, tallow, &c.) not seldom represents a final *g* pure: much rather might it be expected to form the terminus to which *gw* would tend.

≒ Lat. *veh-ere* = Grk *οχ-* (for *Fοχ-*) in ὄχος &c., against Skt *vah-*, Lith. *vež-ù*, O.Sl. *vež-ą*.

This list might be lengthened by examples which compare the Græco-Italian forms on the one side with the Aryan on the other; but as *h* is the terminus to which the Skt Aspirate *gh*, irrespective of any affection, naturally tends, it would manifestly be unsafe to institute such a comparison. I have consequently selected only those examples in which the Aryan forms are supported by all the known forms of the Li-Sl. dialects.

64.—(*a*) One other bearing of the question discussed in §§ 55—63 may deserve a few remarks; and they shall be the last on the subject. I refer to the possible connexion of the affections of the I-E. *k*, in their incipient form, with the movement which gave rise to the vowels *i* and *u*. In the view which would trace both of these vowels to *a* as their parent (§ 6 *antè*) I feel bound to concur; for besides other and more definite reasons, all the glimpses I have obtained of the early history of articulate sounds suggest the general conclusion that in all cases in which feebler Speech-sounds are naturally derivable from other and stronger related sounds, they were once actually so derived[1]. But the phonetic distance, so to say, between *a* and either *u* or *i* seems too great to be covered by a single leap (§ 61 (*d*)); and there is reason to suspect that *a* must have passed to each of those

[1] If the above conclusion may be laid down as an axiom, we shall perceive it to be an indispensable preliminary to any rational inquiry into the Origin of Language (or Languages) to eliminate all secondary or derived sounds from the primeval phonetic system or systems of which we are in search (§ 26, *note*). A minute examination and careful comparison of the linguistic force of the four or five remaining sounds, as they occur in a series of related languages, may then suggest the way in which these sounds originally became vivified with meaning. It has long seemed to me preposterous (I use the word in its strict etymological sense) to assume, as some do, when professing to pursue such an inquiry, the right of employing *ad libitum* the copious phonetic resources of mature languages, and of laying down, as the *primordia* of speech, sounds and combinations of sounds as to which it should first be asked whether they were possible at the birth of language.

feebler forms by some intermediate stage. The two stages are, in fact, those supplied by Fick; who maintains, without reserve, that *i* and *u* are contractions for *ya* and *wa* respectively[1]. By this, of course, is not meant that *a* and its derived vowels appeared or existed in a detached and independent form, as we see them in modern alphabets, but that in certain of the oldest I-E. vocables (call them "words" or call them "roots"), which, like all the rest, were characterized by the vowel *a*, this vowel descended, through the phonetic stages *ya* and *wa* to *i* and *u*. Thus, putting *κ* for *any* consonant, and supposing such vocables to have been originally of the form *κa*, or *aκ*, or even *κaκ*, then their intermediate palatized forms would be *κya* or *yaκ* or *κyaκ*, leading at last *κi* or *iκ* or *κiκ*; and their intermediate labialized forms would be *κwa*, or *waκ*, or *κwaκ*, leading at last to *κu* or *uκ* or *κuκ*. If now *k* be put for *κ*, we obtain, in *kya* and *kwa*, combinations identical to the eye (§ 61 (*d*))

[1] W-B., p. 1043. "Da sich uns durchweg das Resultat ergeben, dass die Zeit der Wurzelschöpfung der Entstehung von *i* und *u* vorausliegt, sind die . . . Wurzeln *i* und *u* in dieser Gestalt nicht zu dulden; ihre wahre wurzelhafte Form ist *ya* und *va* [Anglicè *wa*], woraus *i* und *u* bloss verkürzt sind. Dies wird unumstösslich bewiesen durch den Umstand, dass in den alten und Zahlreichen Weiterbildungen von den fraglichen Wurzeln aus, nicht *i* und *u*, sondern *ya* und *va* als Radicaltheil erscheint; woraus zu schliessen, dass, als diese Determinationen vollzogen wurden, noch *ya* und *va* gesprochen wurde." In these views the student of Gothic, no less than the student of Sanskrit, will find it easy to concur. Anyhow, the vowels *i* and *u*, in their origin and history, cannot (so intimate is the relationship) be considered apart from the semivowels *y* and *w*. And that the latter sounds should be eliminated from the "primeval phonetic system" (See the preceding note) seems to me clear, both for other reasons, and especially from their history in the Greek dialects. For the multiplicity and diversity—as well as, in some cases, the violent nature—of the methods by which those dialects, when located by themselves, proceeded to get rid of both the semivowels, plainly suggest that these sounds could never have been indigenous to the (prospectively) Hellenic subsection of the Holethnos, and that they never took firm root therein: their fluctuating value and strange affinities mark them as foreigners. If, then, the sounds in question were once wanting in any part of the Holethnos, it follows, of course, that they must likewise, at a still earlier period, have been wanting in all the other parts; and that both they and their related vowels, although primitive, were not primeval.

with those representing the phonetic combinations which gave off the Aryan and the European *k* respectively; as, for example, in the Aryan *çad-* (from *kyad-*), "*ced*-ere," and the European *kwa-*, "*who*." May not therefore these affections of *k* fall, as particular cases, within the much broader stream of vowel-change, whereby *i* and *u* were evolved from *a*? It seems to me that they may; nor will the apparent difficulties in the way of such a connexion very obstinately resist removal.

(*b*) The first of these difficulties arises out of the widely different destinies of the combinations *kya* and *kwa*, according as the palatalism and gutturalism represented by *y* and *w* attack the consonant or the vowel. In the one case we arrive at *ça* and *qua-* (or *pa-*), in the other at *ki-* and *ku-*. It is clear, therefore, that the original *phonetic* values of the combinations, notwithstanding their identity to the eye, were not precisely the same in the two cases; and the difference apparently depends on the side to which the semivowels incline. In the one case their inclination is towards the consonant, and may be represented by $ky+a$ and $kw+a$; in the other, their inclination is towards the vowel, and may be represented by $k+ya$ and $k+wa$. Hence, to permit of a common origin, we must suppose, in accordance with § 34 and § 35 *note*, that some slight difference of pronunciation originally "shunted" similar pairs of combinations of sounds on to two different lines of descent. But a second and more serious difficulty arises apparently out of my own hypothesis respecting the impure *k*-sounds. For, by this hypothesis, the Palatal affection (*ky*) is Aryan only, and *not* European, and the Labial affection (*kw*) is European only, and *not* Aryan; whereas, of the corresponding vocalic affections, the palatal (*ya, i*) is as much European as Aryan, and the labial (*wa, u*) as much Aryan as European; and they are so, in each case, no less after *k* than after any other consonant.

(*c*) But if, in philology, we may claim a modest share in the privilege (so largely assumed by the biologist, the geologist, and others) of drawing upon past time to an indefinite amount, we may perhaps find in the different dialectic condi-

tions of the Holethnos at different periods an explanation which, if it does not necessarily connect the consonantal with the vocalic affections, will at least remove their apparent hostility. Treating first (this claim being admitted) the latter of the foregoing objections, we have to admit that, as we have already (§ 61 (*e*)) assigned the Palatal affection, in the case of *k*, to the (prospective) Aryans, and the Labial affection to the (prospective) Europeans, we are bound by consistency to make a similar distribution of the corresponding vocalic affections also; *i. e.*, *ya* (afterwards *i*) must be supposed to have been originally Aryan, and *wa* (afterwards *u*) originally European. Hence each of these incipient dialects must be supposed to have *adopted*, or have attempted to adopt, the vowel-variation of the other, and that, too, in precisely the *same* instances, and not (as happened in respect of the consonantal affections) in different and diagonally related portions of their original and common vowel-series. In accordance therefore with § 28 (*c*, *init.*), we must further suppose that these two primary dialects of the Holethnos had not yet loosened out into that condition in which Mutual Dissimilation becomes possible—in other words, that the era of the vocalic affections must in its turn have preceded, wholly or in part, the era of the corresponding consonantal affections, just as the latter era preceded that of the changes represented by Grimm's Law.

(*d*) This relative chronology of the two smaller movements (or, rather, as we may now perhaps say, two successive stages of the same movement) may obviate the former of the foregoing objections likewise, by suggesting *why* the earlier stage was vocalic only; for it is highly probable that the consonants, and *k* above all, were originally uttered with a force (§§ 24, 25) which in later times was no longer employed, but which would then suffice to repel any debilitating accretion and to confine it to the conjoined and more impressible vowel. By the time, however, that vowel-change had proceeded to the length we find it did, the consonants would have become more open to attack; pronunciation would perhaps have become

more agile[1], so that contact of the consonant with any accretion would be closer; the dialects, too, would have diverged more widely from each other; and as palatism became more and more virulent on the Aryan side, leaving no further room for a second and non-indigenous affection, the debilitating movement would at length pass into the consonantal stage already considered.

(*e*) Nor are there wanting phonetic symptoms consistent with such mutual interchange of vowel-variation on the part of the two dialects. For what may be expected to happen in such a case is (§ 28 (*d*) and § 35 *note*) that the non-indigenous sound will not be exactly reproduced, or not produced with its native affinities, in the adopting dialect. And this certainly seems to hold good in reference to the value and affinities of the labial affection on the Aryan side, as compared with its value and affinities on the European side. For where the Aryan *w* did not actually fuse with the following vowel into *u*, we still find it repelled so far from the *k*, and existing so independently of it, as actually to leave it free to fall a victim to the palatal affection likewise (*e. g.*, Skt *çvan-*=κυν-, "dog," *çvi-*="*que*-o," &c.). Besides, the two main Aryan dialects seem to have slightly differed, in respect of the value assigned to *w*, not only from the European but also from each other. Both perhaps gave it a too nearly consonantal value; for in both, while it left *k* independent, it often likewise refused to amalgamate with the conjoined *a*. But this is more especially true of the Zend, in which the *w* actually passed into *p* (*çpan-*, *çpi-*, &c.). And thus a clear distinction between the vocalic and the consonantal Labial affection is indicated; for the former, although adopted on the Aryan side, is always repelled, as it were, from the *k* which precedes it, while the latter, on

[1] This idea is Baudry's, to whose elegant (but, alas! unfinished) sketch it is a pleasure to be indebted. With great ingenuity and probability he would trace the ancient metrical quantity by position to an earlier difficulty of pronunciation; whereas, in modern languages, "la voix a, pour ainsi dire, *fait son éducation*, et l'articulation, devenue plus agile, n'éprouve plus aucune peine à prononcer d'un seul coup deux ou plusieurs consonnes" (p. 13). See also his explanation of Vowel-weakening in Latin on p. 39.

the same side, is entirely wanting[1]. On the European side, however, (where the sound was, so to say, more at home,) the treatment of the vocal affection is, as might be expected, often identical with that of the consonantal affection (*e. g.*, the Lat. "*cun*-ire" but "in-*quin*-are" = Skt *kun-*; "*que*-o" = *çvi*; "*equu*-s" and ἵππος = *açva*; &c.); and so reversely (*e. g.*, the Lat. *cum* for *quum*, *cutero-* for *quotero-* (=ποτερο-); A-S. *cum*-an, H.G. *kom-men*, Eng. *come*, for *quim-* or *quam-*; Grk γυν-ή for γϝαν-α; and many more). Hence both Fick, who maintains a distinction between what he calls "radical" (*i. e.*, I suppose, our con-vocal) *w*, and Havet, who objects to Fick's view on the subject, are both apparently half right. The former seems to have fixed his eye chiefly on the Aryan, the latter on the European treatment of *w* as a vocalic affection. The former, while distinguishing between the phonetic value of the vocalic and that of the consonantal affection, appears completely to disconnect the two; the latter, while maintaining their general sameness, appears to overlook their real historic and dialectic differences[2].

(*f*) In sum, then, the conjectures of the present section amount to this:—that while the Tenues were still the only mutes, and *a* the only vowel, a phonetic (palatal) accretion attacked the latter in one of two incipient dialects (the prospectively Aryan), and a second (labial) accretion attacked it in the other (the prospectively European),—these two accretions, or at least one of them, radiating perhaps from an accretion of intermediate value (*ü*) intended to be a reproduction of the other, and both being the earliest result of slovenly or drawling pronunciation; that in the condition of close dialectic cohesion which still obtained, both of the enfeebled vowels became current throughout the Holethnos, first in their composite, and afterwards generally in their simpler forms (*i* and *u*), and were applied by the linguistic

[1] There is a manifest consistency between these two facts.

[2] It is but fair, however, to M. Havet to state that, in the article just now examined, he has noted "the origin of the groups *kw* and *cw* in the Aryan" as a question for future investigation. Whether he has since taken up the subject I am unaware.

sense to the extension of its powers of expression; that after the one primitive vowel had been in this way extensively debilitated in two different directions, the same palatal affection (which seems to have been by much the more virulent) at length on the Aryan side attacked the guttural Tenuis; and that, the relationship of the two diverging dialects being by this time much less intimate, the consonantal debilitation evoked, first, Phonetic Resistance on the European side, and then Reflex Dissimilation, and all those attendant phenomena which have already been sufficiently discussed in §§ 55–62.

APPENDICES.

A.

On the Affinity of R *in English for the open* A*-sound.*

(*a*) At the Meeting of American Philologists, held at Hartford, Connecticut, in the summer of 1874[1], there was read a Paper, contributed by Professor Whitney (of which I have unfortunately seen only a very meagre sketch), giving the results of a statistical examination of all the sounds, vowel and consonantal, employed in the English language as at present spoken. The object of the Professor was to determine the relative frequency with which the several sounds recur; and out of these sounds he seems to have selected for special treatment that of the old open vowel *ā* (=Engl. *ah*). This sound formed the main phonetic characteristic, and once probably the only vowel[2], of the primitive I-E. language. Towards the epoch of the Separation, it still constituted nearly 30 per cent. of *all* the sounds[3]; but in modern English it has so nearly vanished that the Professor, in his own pronunciation, could discover only 56 of such *ā*'s in 10,000 sounds, or less than $\frac{6}{10}$ per cent.; while, owing to the prevalent "thinning" or debilitation of that *a* before *s* and *n* (*bäsk, chănce,* &c.), he was of opinion that in the popular speech of the United States the open *ā* formed only some $\frac{2}{10}$ per cent. of all the sounds; *i. e.*, out of every 150 primitive *ā*-sounds, 149 have disappeared.

(*b*) There is one point, however, of which the Professor does not seem to have treated, but which has an important

[1] While I was writing this Appendix, which was originally intended to form part of an extended investigation into the general principles referred to in the *note* to § 35 (*a*).

[2] See §§ 6 and 64.

[3] That is, of vowels and consonants together. Of the vowel-sounds alone, the long and short *a* are reckoned by Dr. E. Förstemann (Gesch. i. 21) to have constituted 75 per cent.

bearing on some parts of the foregoing treatise—namely, the influence of the sound represented by our *r*, not only in protecting an open *a*-sound coupled with it, but also in "raising" thereto other and feebler vowel-sounds, particularly that of the dull *e*. This power of *r* (which, after all, is only one manifestation of a much wider influence[1]) will, I fear, in those cases where it has hitherto affected pronunciation *only*, be ultimately counteracted by the pertinacity with which our written and printed language maintains the weaker vowel. Thus, nearly everybody says *clark, sargeant, Hartford, Barkshire, (Lord) Darby,* and *astarn*; but we have to write and print *clerk, sergeant, Hertford,* &c.; and where orthography is fixed and persistent, pronunciation, in the long run, can scarcely avoid becoming assimilated thereto. But where individual volition has had opportunities of making itself felt, —as in the case of proper names and technical terms—there the printing-office and dictionary are set at defiance; so that we have *Mr. Clark, Mr. Darbishire, Mr. Jarman, Mr. Sargeant, larboard, starboard,* and others.

(*c*) To observe this influence at work most vigorously, we must turn to the language of the people. Here many such modified forms have long been current as so-called vulgarisms; *e. g., larning, 'arnings, sartin* (=certain), *consarn* (=concern), *varmint* (=vermin), *sarpent, 'varsity* (=university), and "*sarve* him right." Two or three of these have crept into the slang of better-educated people; but they will hardly creep any higher in our generation, if ever. In some of our provincial dialects we find this effect of *r* to be quite a characteristic. The following words are from a humorous tale in the Norfolk dialect:—*charch, consarned, Jarmins* (Germans), *larned, matarnal, quarlsome, sarch, sarmon, sarpent, sarvice, 'tarmined, tarning,* and *warld*; where it will be seen that the change is determined by the *sound* and not by the *sign* of the vowel preceding the *r* (*s*earch, *t*urning, world, &c.).

[1] See the section on Consonant Influence in Mr. H. Sweet's excellent "History of English Sounds," which, unfortunately, did not come into my hands until long after this Appendix was written. "The most marked influence is that exercised by the *r*. So strong is it, indeed, that in the present English hardly any vowel has the same sound before *r* as before other consonants" (p. 67).

(*d*) Fortunately for the preservation of the open *a*-sound among us, many such "raised" vowels had already been generally accepted by the standard dialect before the days of Dictionary-makers. In some of these the immediate parent vowel was *eo*; as in *carve, darling, farm, far, farthing, hart, heart, hearth, smart, star, starve* (=A-S. ceorfan, deorling, &c.). Of these, however, there were probably in very old times duplicate or dialectic forms involving *ea* instead of *eo*; indeed, such a variation is a known characteristic of the ancient Kentish and Northumbrian dialects. But other words certainly made their first appearance in our language with a close vowel—as *garland, marvel, parson, parsley, tarnish, varnish*. In the case both of the foregoing classes of words and of many more individual examples (as *bark, barley, barn, dark, hark, hearken, harbour, harbinger, yard, tar, mar*), if our orthography had been fixed, say, by Chaucer, we should still be writing *e* (*fer, hert, smert,* &c.). It should be added that here and there, before *rr*, a "raised" sound appears as a short or as a faucal *a*; *e.g.*, Hărry, for Herry, by assimilation from Henry[1] (*cf. arrant, tarrier*, popular pronunciations of *errand, terrier*); quărrel[2] (Lat. querela). In *garner, garnet*, there is metathesis (*cf. granary, granite*). In the so-called vulgarism *lark* ("to have a *lark*, to go a-*larking*"), the long *a* returns the compliment by attracting to itself an *r*. There can be no doubt that this is only a distortion of the old *lac-an, læc-an*, which still lingers in Yorkshire in the form *laik*[3], "to play." The apparent identity of the word with the name of the bird (from which Webster derives it[4]) has led to a curious extension of the vulgarism; so that we sometimes hear of people being

[1] And not, surely, "a rough imitation of the sound of the French Henri," as Mr. Earle supposes (Philol., p. 344).

[2] This word (in which *qu=kw*) really belongs to a group (*warm, swarm, warp,* &c.) in which the affinity of *r* for *ā* is overpowered by the stronger affinity of *w* for *ā*,—closely resembling that of *v* (*w*) for *o* in Latin. The relative potency of such affinities offers scope for much delicate and discriminating investigation.

[3] Identical in form, and nearly so in sound and meaning, with the Goth. *laik-an*.

[4] Thus indicating a disadvantage under which lexicographers must labour who attempt to account for the constituents of our vocabulary without the opportunity of studying our dialects on their native soil.

"out a-*sky*larking"! The sound of *l* also (which was originally in most cases derived from *r*) exhibits with us a similar influence, although in much fewer examples—its more usual affinity being for the faucal *â*. Thus it *raises* a close sound in *alms* and perhaps *behalf* (where, according to some etymologists, the *l* itself is an accretion; *cf. behoof*), and it *protects* the open sound in *balm, calf, calm, half, psalm, qualm, salve*. In *almond* the *l* is again an accretion (*cf.* Fr. *amande*). On the whole, we have here a clear case of counteraction and even reversal of the general tendency to phonetic decay (§ 8 (*b*)) by means of the action of one sound upon another in close contact with it. This special affinity of *r* for *ā*, however, common as it is with us, is by no means universal: and a comparison therewith of its affinities in many other languages suggests that the English tendency is due to one of those slight differences of physiological formation referred to in the *note* to § 35 (*a*).

(*e*) To obtain something like a definite valuation of this influence of *r*, I had already entered upon a statistical examination of a similar character to Prof. Whitney's; but of this I shall here quote only so much as bears upon the relative frequency with which the several values of *a*, apart from other sounds, recur in our literary language. For examples of styles as different as could well be found—the loftiest poetical and the humblest colloquial—I selected the First Book of Milton's "Paradise Lost" and Chapter xiv. in Dickens's "David Copperfield." The former (omitting many foreign names) furnished about 1600 *a*'s of all sorts, and the latter about 1700. The *proportions* of the several *a*-sounds making up these totals were

	Milton.	*Dickens.*
(i)[1] *ă* as in *fate, mare* .	27·9 per cent.	24·1 per cent.
(ii) *â* „ *all, what* .	9·4 „	8·2 „
(iii) *ă* „ *can, royal* .	52·7 „	59·4 „
(iv) *ā* „ *father* . .	10·0 „ (nearly)	8·3[2] „ (nearly).

[1] In each of the first three classes, as we are here not much concerned with them, two or three shades, as it were, of sound are grouped under one symbol.

[2] The word *aunt* occurs about 100 times in the chapter examined. Assuming that it occupies the places of other *a*-words (of which, on the

The difference of the styles, and Milton's unrivalled command of all the phonetic resources of our language, being considered, the smallness of the difference between the two series of proportionate values is very remarkable. The short and dull *ă*, as might be expected, is, in Dickens's column as compared with Milton's, swollen to some extent at the expense of all the others; but this may be almost entirely accounted for by the incessant recurrence of the conjunction *and*.

(*f*) Pursuing the comparison of the sounds under class (iv) a little further, I found that in Milton about 158 recurrent sounds are distributed over 54 words, while in Dickens about 140 sounds are distributed over 50 words; and out of these 54 and 50 words, respectively, 22 (representing no less than 98 recurrent sounds in Milton, and 69 in Dickens) are common to both. Again, if the same words be divided with reference to the connexion of *a* with *r*, there appear, of the 158 *ā*-sounds in Milton, 88 "protected" (of which 19 have been "raised") and 70 unprotected, and in Dickens 69 protected (of which 12 have been raised) and 71 unprotected; that is to say, somewhere about half the open *ā*-sounds are now protected by *r*. One may therefore venture to predict that this protected series represents the minimum limit to which the number of such sounds may some day fall, and that unless (as, indeed, is by no means impossible) some change in the organic or physiological value of *r* should gradually take place (§ 35, *note*), the prospective debilitation of *ā* will stop when it reaches that limit, if not before. Thus it will at some future time be a curious fact that the preservation of nearly all the open *ā*-sounds in our language will have been due to the extraneous influence of *r*. It is worth while to remark in conclusion that whereas in the part of Dickens examined the verb *are* occurs a dozen times, in the part of Milton it occurs but once. For nearly up to Milton's time, as is well known, the whole of the present tense in which *are* is now found was furnished by the form *be*[1]. We do our best, however, to neutralize this

Miltonic scale, 10 per cent. should be *ā*-words), I have reckoned it in only 10 times. If the word were rejected altogether, there would be left in this class a percentage of only 7·8 under Dickens.

[1] "If thou *beest* he...." (verse 84). So always in the older writers; *e. g.*, Sir John Cheke:—"They *be* faithful at this day, when ye *be* faith-

advantage by eliding the initial *a* in reading and speaking (*we 're, you 're, they 're*), unless the verb is emphatic or interrogative. Nevertheless, on the whole, there is from this source a considerable phonetic gain.

B.

On some current Opinions respecting the Indo-European Aspirates.

(*a*) The brief description of these sounds inserted in § 6 was adopted after a careful examination of all the evidence I could collect respecting them; but it is, of course, almost impossible now to do more than determine certain limits within which their original phonetic value probably lay. There are, indeed, philologists who pronounce upon the nature of these sounds as decisively as if they had themselves been members of the Holethnos; but there are others who, while accepting the Skt Aspirates as the best and indeed the only genuine representatives of the primitive sounds, speak of the latter with a hesitation justified by the obscurity which, after all, rests upon their origin. It is certainly becoming to exhibit such hesitation as to a point on which the old Hindoo Grammarians themselves are not quite so clear as one could wish[1]. I have already tried to show, however (§§ 32, 33), what seems to me reason for questioning the views most prevalent both as to the dialectic parentage of the Aspirates and as to the tendency of these sounds to descend to Mediæ. This necessarily shifts the ground on which their original nature has to be determined. Nevertheless, on one point we ought all to agree; viz., that, whether the Media or the Spirant be the goal to which the Aspirate naturally tends, the first stage of Aspiration, although the memory of it may have perished, and its reproduction be impossible, can scarcely have been anything else than an incipient debilitation.

(*b*) But it is just here that the oracles utter dark sayings. With Grimm[2], the Media is first "thinned" to the Tenuis and then "thickened" to the Aspirate. Curtius's opinion is

less, not only to the king, whose subjects ye *be*, but also to your lords, whose tenants ye *be*."

[1] Max Müller; Lectures, ii. 164, 165. [2] Gesch., p. 416.

decided that the Aspirate was an "Affection of the Tenuis"[1]. Grassmann[2] also considers the *Hard* Aspirate to have been directly derived from the Tenuis. Schleicher[3] (advancing halfway to the conclusion of § 25 *antè*) would strike out the Aspirate altogether from the sounds of the *Ursprache*; and so would Prof. Max Müller[4]. The latter allows, indeed, that the "Aspiration of the Hard" was "the beginning of a phonetic infection"; but his "infection" apparently means "invigoration"; for not far off we find the Professor figuratively describing the Aspirate as "the boldest of the bold", and representing it as the product of a "raised" Tenuis. If this really means that **H** is to be considered the weaker and **A** the stronger sound, it will, of course, greatly extend the ground of opposition between the Chronological Hypothesis of Grimm's Law and the Principle of Least Effort. But I can hardly think that such a view of Aspiration is the correct one. For if we bear in mind the ultimate forms to which the Aspirates are *known* to tend—*f*, *v*, *þ*, *ð*, and *h*; which last (as in modern Italian and practically in French) has sometimes completely vanished,—the description of the earliest form of **A** (whatever it may have been) as a "raising" of **H**, the purest and firmest of the mutes, will sound in our ears pretty much as if one should declare that the earlier stages of a disease ending in exhaustion and death are but the signs of a more robust health. In point of fact, the two *termini* of the sounds in question seem to me to furnish the only, or at any rate the most trustworthy, evidence on which to base a conclusion as to their former phonetic value and relative strength. For if they started from the complete contact of the Tenuis, and if they have descended to more or less open and unimpeded breathings, the Uniformity of Nature and the Principle of Transition require us to believe that the characteristics of all their intermediate stages must have lain between these two extremes. To assert the contrary would be to give to the line of phonetic development precisely opposite directions in different parts of its course.

[1] Grk Et., 3rd ed., p. 437; but his hypothesis (§ 12 *antè*) really makes the primitive Aspirate a *twofold* affection—by softening as well as by aspiration.

[2] Kuhn's Z-S., xii. 81, &c. [3] Comp., p. 11. [4] Lectures, ii. 222.

(*c*) With respect to the almost universal assumption that the old *Sanskrit* Aspirates were the exact representatives of the primitive Aspirates, I shall here add little to what has been already said in the text (§ 33, *et alibi*). If that assumption were indisputably true, several considerations suggest that the prevalent deductions therefrom should be received with caution and tested with care. Our first business, of course, would then be to make sure that we had actually got hold of the most ancient Sanskrit Aspirates. Now the only means to this end are furnished by the descriptions of the old grammarians and by the pronunciation of the modern Brahmins. As to the former, even if those descriptions were in some points less obscure than they are, it would still be scarcely possible for us moderns and foreigners (§ 35 *note*) to reconstruct therefrom the *exact* sounds described; and the more elaborate the descriptions, the less, perhaps, our chances of success. If we turn to the latter, there arises the question how far the modern Indian pronunciation correctly represents the ancient sounds. Considering the general liability to change of the Aspirates in cognate tongues, one would rather not assert that even this representation is exact. It may be granted that the mode of transmission of Sanskrit has been peculiarly favourable to the preservation of its phonetic system. But, without stopping to inquire whether a *quasi*-artificial tradition of sounds may not be subject to dangers of its own, it seems to me scarcely possible but that the changes of vernacular in India during thousands of years should have affected the training of the Vocal apparatus of the natives, Brahmins included, and, in spite of and unperceived by themselves, should have exerted a sort of reflex perturbing action upon the sounds of the traditional language.

(*d*) But, once more, the phonetic values currently assigned in Europe to the existing Skt Aspirates are generally determined for us by our fellow-Europeans, and only indirectly by the Hindoos. The effect of this circumstance also must be taken into account when great exactitude is in question. In the case of ordinary men, their own belief that when speaking another language they exactly reproduce its sounds (where these differ to any extent from the sounds of their own language) is often an amiable delusion; and not seldom their ear

altogether fails to appraise some sound or other at its proper phonetic value[1]. A closely related mistake is to suppose that sounds which do not occur in our native tongue, and which we therefore (especially in mature or advanced life) find difficult to acquire at all, are in themselves essentially difficult. One's opinions on such points are, *malgré soi*, moulded by one's sensations; for the *practical* ease and difficulty of sounds depends greatly on the previous education of the vocal organs; and where we too readily employ the terms "easy" and "difficult" we should rather say "familiar" and "unfamiliar". The great Sanskritists of Europe tower, indeed, above ordinary men; but they would probably themselves be very backward in claiming exemption from the inevitable operation of physical laws.

(*e*) To be brief, the points on which, as it seems to me, the current views must leave their adherents in doubt are:—(1) whether the ancient Skt Aspirates may not themselves have differed from the earliest I-E. Aspirates (§ 33 *antè*); (2) whether the Aspirates of the modern Hindoos again may not in turn differ slightly from the ancient Skt Aspirates; and (3) whether the pronunciation of these modern Aspirates and the estimate of their comparative difficulty by Europeans do not almost necessarily differ from those of the Hindoos themselves. However minute these successive differences may be, they suffice individually (and still more collectively) to affect the judgment when great phonetic exactitude is required, as, for example, when we are considering the somewhat delicate questions:—whether Aspiration was, at its inception, a *debilitating* affection or not; and whether or not the breathing, from which the Aspirates get their designation, was then an *inseparable* element in their composition, as it still is in that of their descendants the Spirants. I cannot now enter into the further inquiry whether, and how far, the accumulation of minute errors, like those suggested above, may have influenced the choice, or at least the general acceptance, of the composite symbols (*kh*, &c.) by which in Europe the modern Indian sounds are represented. But as to the converse question, how far the prevalence of these

[1] See the amusing case of *bactshtasch* (*bakhshish*) quoted by Max Müller, Lectures, ii. 186.

symbols may influence men's ideas of the sounds, I remark, in conclusion, that, by their very composition, they *seem* to give, to each of the two delicate phonetic questions just stated, an answer which I can scarcely believe to be, in either case, correct.[1]

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C.

On the Extension of Language by Phonetic Variation. (*Opinions and Illustrations.*)

(*a*) The process referred to in § 17 (*d*) and formulated in §§ 28–30, is too well known to need further theoretical treatment. I only propose, in this Appendix, to fortify my position by the authority of two or three writers whose opinions will hardly be gainsaid, and by a few familiar examples that may suffice to exhibit clearly the leading characteristics of the process.

Bopp remarks:—"Die Spaltung *einer* Form in verschiedene mit grosserem oder geringerem Unterschied in der Bedeutung, ist in der Sprachgeschichte nichts Seltenes" (V. G., 3rd ed., i. 33). And again:—"Hierbei hätte man zu berücksichtigen dass in der Sprachgeschichte der Fall nicht selten vorkommt dass eine und dieselbe Form sich im Laufe der Zeit in verschiedene zerspaltet; und dann die verschiedenen Formen vom Geist der Sprache zu verschiedenen Zwecken benutzt werden" (Id., ii. 391).

Prof. Key is particularly felicitous in his illustrations:—

[1] As an illustration of the dangers which beset us when we attempt to represent to the eye the sounds (sometimes even the simplest sounds) of an alien tongue, with which we suppose ourselves perfectly familiar, and of the mistakes which we may thus propagate, I cite the following fact:—A French "Professor of English" recently constructed an elaborate system of symbols for conveying to his countrymen correct ideas of our English sounds; and among them I find these two—ʃts and ʷdz. When the reader is tired of guessing what these can mean, he may like to be informed that they are intended to denote the two values of "le *th* anglais"! The ingenious inventor certainly does his best to neutralize the appalling effect of his symbols by an elaborate verbal description of our Dental Spirants. Nevertheless what Frenchman, beholding those combinations, could possibly conjecture that these spirants are the simple, feeble, and to us easy sounds they are?

"When a word has established itself in several dialectic varieties of form it is a great convenience to distribute any varieties of meaning which may belong to the parent word between them; and thus a dissolution of partnership, as it were, takes place, each dialectic variety commencing business on its own account with its own separate stock." ("Philol. Essays," Essay I.)

Again, Fick :—"Auch ist ja bekannt mit welchem Heisshunger sich die Sprachen auf neu hervortretende Lautspaltungen werfen, um dieselben als Träger von Bedeutungsdifferenzen sich nutzbar zu machen." ("Sprachcinheit," p. 202. See also p. 210 for some valuable illustrations.)

Or, as Dr. E. Förstemann says, "Ein Wort hat eine so grosse Begriffssphäre dass dieselbe für die Sprache zu lästig wird; da pflegt ihm denn ein neues Wort einen Theil dieser Sphäre abzunehmen." (Gesch., p. 224.)

So too Whitney (Jolly), p. 175 :—"Nicht selten tritt der Fall ein, dass einer neuen Schattirung der Wortbedeutung eine Differenzirung der Lautform zu Hülfe kommt"; and numerous examples are quoted from the German (*e. g.*, *nah* and *nach*; *können* and *kennen*; *Geist*, *Gas*, and *Gischt*; *opfern* and *opferiren*; &c.).

(*b*) English abounds in such cases, the invasions and powerful phonetic influences to which it has been exposed having been particularly favourable to their production. Many of these have diverged phonetically and differentiated their meanings almost, as it were, under our eyes. Here, for example, are some variations by simple vowel-change :—A b*a*nd, a b*o*nd; b*o*rne, b*o*rn, b*ai*rn; ch*ea*p, ch*a*p-man, ch*o*p, ch*a*ffer; to d*a*b, to d*au*b; humăn, humāne; kĭn, kīn-d (=genus), kĭn-d (=benignus); m*oo*d, mode; p*e*rson, p*a*rson; p*ie*ty, p*i*ty; a pl*ai*t, a pl*ea*t, and (O.E.) pl*i*ght; r*i*se, r*ai*se, r*ou*se; sc*a*r, sc*o*re, sh*ea*r, sh*a*re, sh*i*re, sh*o*re; to s*i*t, s*e*t, s*ea*t; a st*i*ck, st*a*ke, st*o*ck; thr*ou*gh, thor*ou*gh; w*i*ght, *whit*, *au*ght; &c., &c. The varieties produced by modification of the consonant are perhaps still more numerous. In the case of some, indeed, the logical variation is but slight, as in *d*rill, *th*rill; owe*d*, ou*gh*t; *c*arl, *ch*url; ca*n*on, ca*nn*on. In others the least phonetic variation is made to indicate, for example, different parts of speech, as, advice, and to advi*s*e; belie*f*, and

to believe; gloss, gloze; lose, loss, loose; to use (*s*=*z*), and use. The same result may even be attained by merely shifting the accent, as in august', and Aúgust; per'fume, and to perfúme. Some of the consonantal variations in English very closely resemble the debilitations to which I have attempted (§ 43) to reduce the so-called *Lautverschiebung*; *e. g.*, dike, dig, ditch; deck, thatch; wake, [wag?], watch; stick, stitch; hang, hinge; milk, milch; drag, dredge, drudge; read-y, rath-er. In the transition from ancient and irregular inflexions to more modern and regular ones, some of the former, instead of being allowed to die out, are seized and diverted to a cognate use. This is particularly the case with the old passive participles in *-en*. Thus, bounden, drunken, (for)lorn, molten, shaven, shorn, shrunken, stricken, (un)washen, are now generally used as simple adjectives. But the most remarkable examples of variation are furnished by those words which have radiated, with a moral significance, from proper names. Thus, "brummagem," a popular pronunciation of Birmingham, is or was often used, for a well-known reason, as an adjective = "spurious" (*e. g.*, of jewellery, &c.); so, too, from Bethlehem (Hospital) we get "Bedlam" as applied to any scene of uproar and confusion, and from Magdalen "maudlin."

(*c*) These are only a few of the results of what one might almost call the instinctive artifices resorted to by the Linguistic Sense, in order to bring or keep its powers of expression well abreast of the incessant additions to our ideas. In other languages we meet with expedients that seem (although they really are not) still more artificial. Such, for example, in French, is the variation of meaning attributed to certain adjectives according to their *position* before or after their nouns, and the assignment of different *genders* even to nouns of one and the same origin (*aigle, manche, pendule*, &c.); in which latter case the means of distinction have to be supplied by the accompanying article. Somewhat similar to the former of these "artifices" is the English practice in reference to what are called "prepositional" verbs. Thus a country is *over-run* by the enemy; but a person is *run-over* by a wagon; a cup, pail, &c., is *up-set*; but a mast or monument is *set-up*; a swift horse *out-runs* a slow one; but water *runs-out* of a vessel; and so on.

(*d*) But perhaps the finest field in which the Differentiating Process may be observed at work, at least in modern languages, and preeminently in English, is the desynonymizing of words once identical in meaning. The materials on which it here operates are not, indeed, supplied by dialectic variations of sound; but the nature of the process and its logical effects are precisely the same. For closely related dialects we have now to substitute distinct languages, or dialects remotely akin, which extraneous causes have brought together, and which contribute to the resultant language two or three totally distinct expressions for one and the same general idea. Many of these duplicate and triplicate forms of course die out, the corresponding ideas being sterile (§ 30). But those that are retained gradually become assigned to diverging varieties of meaning, in just the same way as the mere phonetic by-forms quoted above. Hence the description of the action of language in respect of these so-called synonyms is couched in almost the very same terms as those employed to describe variation by Phonetic Differentiation. "All languages tend to clear themselves of synonyms as intellectual culture advances, the superfluous words being taken up and appropriated by new shades and combinations of thought evolved in the progress of society"[1]. This division of the subject, however, has already been sufficiently elaborated by others[2]. I only refer to it with the view of strengthening the evidence for the operation of the vigorous linguistic process under consideration. It merely remains to argue, in conclusion, that if the phonetic process is so active in our own era, among mature languages, whose vocabularies are already so copious, and which have availed themselves so largely of one another's resources, surely the same process would be very much more active in the infancy of a language, whose vocabulary was yet, in great part, to construct, and which had to find all its resources within itself.

[1] De Quincey: quoted by Archbishop Trench in his Lectures "On the Study of Words," Lect. v., *note*.

[2] See the admirable Lecture just referred to; or Lect. xxvi. (Smith's edition) of Marsh's "Lectures on the English Language."

D.

On Mr. Sweet's Scheme of the "Lautverschiebung."[1]

(*a*) It is a misfortune for the subject I have been attempting to handle, that Mr. Sweet has touched upon it merely, as it were, by the way, and in subordination to an individual doctrine which, if true, ought contrariwise to be shown to follow from a correct *rationale* of Grimm's Law. The doctrine referred to is, in its author's own words, that of the "Vocal character of the *Thorn*"—a phrase which, expanded, means that, among the Low Germans in general and the Anglo-Saxons in particular, there was originally but *one* Dental Spirant, and that that one was the *Soft* or Voiced Spirant (*dh* = *th* in *th*ou). It is a necessary extension of this doctrine that the "vocality" attributed to the Dental Spirant must likewise be predicated of the Labial and Guttural Spirants, as well as of certain pairs of Sibilants; but the bulk of Mr. Sweet's remarks have reference to the Dental Spirant only. The arguments in favour of the *unity* of this Spirant are:—that in Gothic it is always represented by a single character; that the earliest Old-English (*i.e.*, A-S.) MSS. severally employ one or other of the two characters (þ or ð) throughout, and not both; and that in H.G., both old and modern, the L.G. Dental Spirant has the single representative, *d*. The arguments in favour of its *vocality* are:—that the Latin digraph *th*, which appears in some of the oldest remains of our language, and which should represent a hard or breath sound, was speedily abandoned as if unsuitable; that the symbols þ and ð seem to be both derived from the letter D (*d*); that the L.G. Spirant is represented by *d* in H.G.; "and, in some of the oldest documents verging towards L.G.," by *dh* in all positions. These are all matters of fact. But to derive the L.G. Soft Spirant directly from the Cl. Tenuis (as is required by the Historical or Chronological Hypothesis of Grimm's Law, which the author adopts) would be a departure from the Principle of Transition[2]; for there would manifestly be

[1] Appendix I. to "Gregory's Pastoral Care" (edited for the Early English Text Society); in connexion with which should be read the "History of English Sounds," issued by the English Dialect Society.

[2] See *ante*, § 54 (*f*), *note*.

both loss of energy and breach of contact[1] at one and the same time. Hence, considering that δ in modern Greek, *d* medial and final in Danish and Old Norse, and sometimes (apparently) *d* medial in English (*e. g.*, fa*d*er, mo*d*er[2]) have passed into Soft Spirants, Mr. Sweet feels justified in assuming "an earlier stage of the Teutonic languages, in which the old Aryan *t* was changed into *d*; whence the later (*dh*) arose from imperfect stopping." Applying therefore (*note* to §20(*a*) *antè*) the process which I have designated Cross Compensation, he constructs the following scheme of *Verschiebung* for the dentals :—

(A)

Old Aryan	*t*	*d*	*dH* (Soft Aspirate).
Oldest Teutonic . . .	*d*	*t*	*dH* (Soft Aspirate).
Oldest Low German .	*dh* (Soft Spirant)	*t*	*d*
Oldest High German.	*d*	*tH* (Hard Aspirate)	*d*, *t*

(*b*) It might seem to be intended that the O.H.G. system should be derived at once from the Oldest Teutonic; to which it clearly holds the same general relationship as the Oldest L.G. system. But I have authority for stating that we are to supply a stage intermediate to the third and fourth of the foregoing systems; so that the O.H.G. is supposed to be derived from the O.L.G. system by two stages, in just the same way as the latter from the "Old Aryan"; only, by a slight variation of order, *d* in O.L.G. is first to remain stationary, while *dh* and *t* are to exchange places. Hence, with the necessary extensions to the gutturals and labials, and using our own notation (§ 20 (*a*)), we arrive at the following general scheme :—

(B.)

$$\left.\begin{array}{llll} \Sigma_x & = \mathbf{H} & \mathbf{A} & \mathbf{S}, \\ (\Sigma_{x'}) & = \mathbf{S} & \mathbf{A} & \mathbf{H}, \\ \Sigma_y & = \mathbf{A} & \mathbf{S} & \mathbf{H}, \\ (\Sigma_{y'}) & = \mathbf{H} & \mathbf{S} & \mathbf{A}, \\ \Sigma_z & = \mathbf{S} & \mathbf{H} & \mathbf{A}. \end{array}\right\}$$

[1] See § 6. [2] See the provision made for these words in § 49, *note*.

(*c*) This scheme certainly represents a bold and original extension of the Historical or Chronological Hypothesis of Grimm's Law; and if that hypothesis were demonstrably true so far as it goes, every honest inquirer would hail this extension of it as a closer approximation to the ideally perfect truth of which we are all in search. But, as things stand, there is reason to fear that any extension of the said hypothesis may only multiply some or other of those doubtful characteristics to which I have been unfortunately compelled to direct attention. From my point of view such fear seems to be justified in the case before us; and so, as the scheme appears only to have been put forward tentatively, as it were, and in order to support a doctrine which is itself open to vigorous controversy, I need hardly perhaps apologize for hesitating to believe that its claims to acceptance have been made good. I defend my hesitation by a reference to the objects I have striven to attain throughout the foregoing inquiry, viz.:—to harmonize Grimm's Law with the Principle of Least Effort; to reduce the phonetic movements it really represents under known linguistic processes; to show that the functional relationship of Σ_x, Σ_y, and Σ_z follows necessarily from the conditions of their evolution; to explode the idea that the German Mutes have been of a more fluctuating nature than those of other Indo-European peoples; and to construct a theory that should embrace and explain *all* the phenomena represented by the Law, and that should, if possible, reduce its apparent complexity to simplicity, and not render it still more complex.

(*d*) With these objects the acceptance of the scheme now before us is quite incompatible. If it is an objection, for example, to the Chronological Hypothesis pure and simple that it twice appears to offend against the Principle of Least Effort, much more will it be an objection to this extension that it appears to offend four times over[1]. The "Cyclo-functional" relationship indeed (§§ 2, 3) of the three Mute-systems does not now offer much difficulty; but that is only because it is completely destroyed; that is to say, Σ_y no

[1] It may be said that the application of Cross Compensation to the scheme removes this objection; but I doubt (§ 20 (*c*)) whether the other features of the scheme justify such application.

longer appears to be derived from ($\Sigma_{x'}$) by the same amount and kind of phonetic change as ($\Sigma_{x'}$) from Σ_x; and so of the rest. Again, as to the nature of the mutes, instead of stability we find perpetual flux. Let us trace, for example, the supposed metamorphoses through which the primitive Tenuis[1] must pass before it reaches the final H.G. stage, and let us seriously ask ourselves whether it is possible that the very bones of articulate speech should be reduced to such a pulpy and fluctuating condition; or is it like the economy of nature that the framework of language should be repeatedly broken up and rearranged with the feeble result of bringing its various parts again and again into one and the same position? If we contrast this supposed transmutation or repeated revolution with the acknowledged quietude and stability of the liquids and vowels, the more impressible elements of speech[2], we must, I think, hesitate to believe in it except upon a cogency of evidence amounting to the clearest demonstration.

(*e*) But even this amount of change does not account for all the phenomena of the case. There are, in the first place, some anomalies of phonetic generation, so to say, yet to be explained. Not to mention the various ways in which every sound seems capable (Scheme (B)) of directly generating every other, and indirectly itself too, we observe in the author's own scheme (A) that the Aspirate exhibits in the successive systems, widely different phonetic powers or qualities. It is, in the first two, a Soft Aspirate, in the next a Soft Spirant, in the last a Hard (but spurious) Aspirate; and we are to imagine not only that each of these different phonetic quantities may directly generate or be generated by either of the others, but also that they may stand as compensatory equivalents to one another. Against the passage of the Soft Aspirate to the Soft Spirant (which is the natural line of debilitation) nothing, indeed, can be urged—if, at least, the O.L.G. system (Scheme (A)) be supposed to have been *cut off* from the imaginary Old Teutonic some length of time previous to the evolution of the next following system. But that the O.H.G., in merely in-

[1] Scheme B above; where it will be seen that **H** is supposed first to have become **S**, then **A**, then **H** again, then **S** again.

[2] The reader will have noticed that all the examples of Cross Compensation quoted in § 18 are furnished by the feebler sounds.

terchanging the Spirant (which it already possessed) with the Tenuis, should also have altered its nature and increased its strength, is contrary alike to the Principle of Least Effort and to what we observe as taking place in Cross Compensation. Attention has already, however, been directed (§ 20 (*c*)) both to this last point, and likewise to the disappearance of some of the systems which must once have stood as the fixed and standard systems, and against which some of the fluctuating systems must have differentiated themselves[1]. Leaving these points, therefore, I only remark further that in the labial and guttural series, the earliest *extant* form of Σ_c, as we have seen (§§ 16 (*c*), 52 (*e*)), gives us—not *p*, *ph*, and *B*; *k*, *ch*, and *G*, at which we ought to arrive by this scheme, but—*p*, *ph*, and generally *F*; *k*, *ch*, and generally *H*. Of this phenomenon the scheme provides no explanation. By what intermediate stage, or by what mode of compensation consonant with the Chronological Hypothesis, these odd Spirants are made to occur in these places, it is not easy to see; so that, in spite of its complexity, this scheme, after all, leaves us in

[1] In doubting (§ 20 (*c*)) the possibility of a complete absorption of the standard by the fluctuating dialect, so that no trace of the former shall anywhere be found, I would not be understood to assert (what would be absurdly wrong) that the latter can make no impression of any kind upon the former. Stray examples of the process of Cross Compensation (like irregularities of Aspiration (§ 18 (*d*)) may, of course, in rude times, easily force their way into a more stable dialect. It is in this way, perhaps, that a short but striking list of exceptions in French derivation may be explained. Everybody knows that the Latin internal *b* becomes *v* in French, that initial *v* remains *v*, and internal *v* disappears; the examples of which changes are very numerous. Yet in some half-dozen words (See Brachet, Dict., *s. v. bachelier*, and Introd., p. 93, *note*) *v* is unaccountably strengthened to *b*. M. Brachet (*ubi sup.*) asserts, indeed, that such exceptions "s'expliquent aisément"; but he judiciously abstains from any "explication" beyond vague generalities. If we suppose, however, that, in the early centuries of our era, while *b* and *v* still maintained their original places in the purer Latin of the upper classes, the debilitation of the former to *v* in the commingled popular dialect evoked a compensatory movement which often raised *v* to *b*, and that some of these *b*'s gradually worked their way upwards, we shall have an explanation which is rigidly precise. M. Brachet involuntarily supports this view by producing evidence to show both that these exceptional *b*'s were in early times much more numerous than now, and also that they were then considered vulgarisms.

bewilderment. If, then, the scheme, like the hypothesis to which it is attached, is really untenable, the rejection of the doctrine which the scheme is intended to explain may be thought necessarily to follow. But as the independent arguments by which it is attempted to establish that doctrine come into contact at several points with my own line of reasoning in the foregoing treatise, I will devote a few lines to a separate examination of them.

(*f*) As to the original *unity* of the Dental Spirant (or of an older sound of which it is a modification) there can be little doubt: so much is requisite in-order that the German Mute-system may be made to conform to the general I-E. type. But to how late a date that unity may have continued —or, what is the same thing, at how early a date a second and weaker Spirant began to be given off—nobody knows. The fact that a single sign is used in Gothic, and either þ or ð in the earlier A-S. MSS., does not, of course, prove that a splitting of the sound had not already commenced. Nay, if we suppose the evolution of the younger sound to have commenced before the Germans had begun to use any one of the symbols which have come down to us, and that they appreciated the difference between the two Spirants, they would still be likely to think it a piece of wise economy to make one sign do duty for two sounds so closely alike, and of unnecessary refinement to invent a second. Even so they would have shown themselves very superior to their English descendants, who, having two most convenient signs ready to hand, have neglected to keep either of them, and have substituted for both the one clumsy and inappropriate Latin digraph *th*. If, however (as it will be safer to assume), the older sign (þ) alone was appropriated to the Spirant *before* the evolution of the younger variety of the sound had made any or much progress, or while (as is usual in the case of linguistic changes) the old and new varieties were still used indifferently, that one sign would continue to be used for both sounds as a matter of course; just as, in modern English, the same written vowel continues, for merely historical reasons, to represent three or four distinct vowel-sounds, even when these sounds, phonetically considered, have other and more appropriate symbols.

(*g*) But the relative age of the younger sound and the older symbol is a point of only secondary importance in comparison with the relative age of the two sounds. It is agreed that the Dental Spirant was originally one, and that in later times a younger variety was evolved from that one. It follows, by the Principle of Least Effort, that the original sound must have been the stronger or more difficult, and the younger the weaker or easier,—and conversely in each case; for, with respect to the Dental Spirants, we can now perhaps only determine the age by the strength, and not the strength by the age. But which *is* the stronger? To this question, till recently, there would probably have been but one answer. It was, in fact, answered in advance by the very epithets "hard" and "soft," by which we distinguish the *th* of "*th*ing" (for example) from the *th* of "*th*ou" and "*th*ee." Mr. Sweet, however, as we have seen, propounds an opinion hereupon which (as he justly apprehends) is scarcely of a kind to meet with acceptance among philologists. It is none other than that what is called the *Hard* or *Breath* sound is really the *weaker*, and requires *less effort* to produce than the *Soft* or *Vocal* Spirant, owing to the contributory action of the *chordæ vocales* during the utterance of the latter. Now, if this were a difference to be settled by weight of opinion, I should not risk mine against the one just stated. But it is not: it is a question of facts. And it is a fact, I believe, in the first place, that the Soft Spirant may be made as voiceless as the Hard; in truth, it is by testing the whispered sounds one against the other that we can best appraise their relative strength. This, however, is by no means essential; for, in the next place, I believe it is another fact that the action of the *chordæ* in the production of voiced sounds, if perceptible at all, is less so than that of any of the other organs or muscles concerned in the process, and, where two sounds would otherwise be of exactly equal difficulty, that action, if called for by one of them, will scarcely cause this one to preponderate in difficulty. The part they play in speech may almost be compared to the part played by the iris of the eye in vision; they act, at least within certain limits, by almost involuntary mechanical adjustment to influences external to

themselves. The effort of speaking is rather to be measured by the degree to which it calls into action the larger muscles, closely connected, indeed, with the vocal and respiratory passage, but yet not themselves strictly "organs of speech." Thus the "back" vowels are stronger than corresponding "front" ones, principally because of the amount of work which the distinct production of them entails upon the maxillary muscles. The calls upon the respiratory apparatus are pretty nearly the same for both; but the former require a considerable hiatus, while for the latter the mouth may be nearly closed. So, in the case before us, it seems to me yet another fact that the diaphragm, and even the abdominal muscles, are, in producing a clearly-marked Hard Spirant, exerted with a vehemence which, in producing the corresponding Soft one, we cannot equal if we would. In passing from the former to the latter, it is the very relaxation of the muscular tension which leaves the *chordæ* free to vibrate; and in changing, reversely, from the "Soft" or "Voiced" Spirants (*v, z, zh,* ð) to the related "Hard" or "Voiceless" ones (*f, s, sh,* þ), the suppression of the vibration will be distinctly felt to be due to the additional vehemence of the general effort. But even the action of the smaller muscles must not be left out of the account. In the production of the Hard or Voiceless Dental and Labial Spirants the pressure of the tongue and lip respectively against the teeth is firmer, and the *rima* through which the breath has to be forced is narrower, than in the case of the corresponding Soft or Voiced Spirants. Indeed the greater resistance thus offered to the passage of the breath is the necessary counterpart of the greater sum-total of force employed in its expulsion. All this is a matter of experiment which the decided difference between the two related Spirants forming each pair allows every man satisfactorily to try for himself. I am afraid therefore we have no choice but to revert to the current opinion that the Hard or Voiceless Dental Spirant is the stronger, and *ipso facto* the older, of the two, and to interpret accordingly all the subordinate evidence adduced by Mr. Sweet. But perhaps even this evidence should hardly be passed over in silence, lest it should be thought to offer

independent support to the opinion I am controverting, and to be irreconcilable with the opposite opinion; which is by no means the case.

(*h*) It is manifest that the question under debate, whichever way it is settled, cannot be settled for one pair of Spirants (as þ and ð) apart from the other pairs (as *f* and *v*, *s* and *z*), the members of which are mutually related in precisely the same way. Indeed Mr. Sweet himself proposes to embrace the lábials, at any rate, under the novel doctrine he lays down. For support he refers to the history of the *u* (=*v*) of O.H.G., which *v* is *now* =*f*, and to the prevalence of *v* for *f* in modern Dutch. The former circumstance has already been embodied in its place in the general theory of this book (§ 54 (*f*)). The value of the latter depends on the answer to the question whether the Dutch pronunciation, which cannot be traced back for more than three or four centuries, is to be taken to represent the "Oldest Low German" in preference to that of an overwhelming majority of the cognate peoples. The affirmative could only be justified by satisfactory independent evidence. On the other hand, over and above the physiological argument of the preceding paragraph, a great deal of evidence for the opposite view might be collected by any one who had leisure for the task. There is, for example, the argument from "protection" and "assimilation" (*e.g.*, thrive but thri*ft*, gi*v*e but gi*ft*, &c.; with which compare *live*d, *shov*ed, *mov*ed, &c.; compare also kic*ks*, smi*t*es, whi*ps*, &c., with wa*gs*, buil*ds*, ro*bs*,—*i. e.* wa*gz*, &c. &c.); from which we may infer that *f* : *v*, or *s* : *z* : : *k* : *g* : : *t* : *d*, &c. Similarly, in Latin, we have *scribere* but *scripsi*, *nubere* but *nupsi*, &c., where *s* is itself the assimilating agent: compare also the *insertion* of *p* in *hiemps*, *sumpsi*, *dempsi*, &c. Reversely, when a protecting Tenuis has disappeared, a Spirant that changes at all changes from voiceless to voiced. Thus in our incessantly used "*is*" the *z*-sound is demonstrably younger than the *s*-sound which the word must have had when it ended with *t* (Goth. *ist*, Lat. *est*, Grk. ἐστί, Skt. *asti*, &c.). Add the case of the soft sibilants in our abstract nouns in *-ion* derived from the Latin (*e.g.*, "invasion," "derision," "prison," &c.), where the *s*'s were originally evolved directly from *t*. With respect to *s* and *z*, indeed, no evidence can be clearer than that furnished by

the Gothic, where the connective and relative enclitics beginning with a vowel, as well as case-suffixes, (*-uh, -ei, -is, -a,* &c.,) often coalesce with cases and stems ending in *s*, with the general effect of turning that *s* into *z*. Thus *hvas* + *uh* gives *hvazuh*; *þis* + *ei* gives *þizei*; *Moses* + *is* gives *Mosezis*; *ans* + *a* gives *anza*; and so on, in instances too numerous to quote. Two things are thus clearly indicated—(1) the relative age of the two Spirants, and (2) the agency (bivocalism) to which the softening of *s* to *z* is due. If, therefore, returning to the Labials, we find a considerable number of words (like *eleven, twelve, glove, heave, stove, over, heaven*) involving a *v* in modern English, but only found with *f* in the older L.G. dialects, we may be sure that the apparent history of the change is the real one, and agrees with the physiological character of the two sounds—that the voiceless *f*, like the voiceless *s*, is the older and stronger; and the voiced *v*, like the voiced *z*, is the younger and weaker. And what is true of these must be true of the Dental Spirants likewise.

(*i*) Such apparently obvious conclusions from no scanty evidence Mr. Sweet's general doctrine compels him of course to repudiate. By this doctrine our Hard or Voiceless Dental Spirants are descendants of older Soft or Voiced Spirants. Hence the supposed "mystery" of the Soft or Voiced *initials* in some of our commonest words (*th*ee, *th*ou, *th*en, *th*ere, &c.) is "solved" by assuming that "these words are simply archaisms — remnants of an older stage of pronunciation preserved unchanged by the *frequency of their occurrence*." But, even granting the "mystery," surely this "solution," from whichever point of view it is regarded, is open to suspicion. For, in the case of an indigenous debilitation, such as is here in question, it is the very words in most frequent use that ought to be soonest and most completely affected[1]; so that if the initial Spirants just referred to were originally Voiced (as they now are), and the natural direction

[1] If the case were one of phonetic attack from without, where one dialect is called upon to *resist* the infection of another in contact with it, the solution above challenged might then hold good. Such resistance ("Retention of the Tenuis") has therefore been properly assumed as the basis for an explanation of the Exceptions to Grimm's Law (§§ 48 (*c*) and 50 (*a*)).

of change ran towards the Hard or Voiceless Spirant, the words involving them should have been among the first to be drawn in by the movement, and should now exhibit the Hard Spirant—which they do *not*. If, on the other hand, those Spirants were originally Hard or Voiceless, and debilitation took the direction currently believed, then the words in question actually *do* exhibit the very change that they ought. On the prevalent (and, I believe, correct) doctrine, therefore, everything is in order, and there is no mystery in the case; for the softening of the Dental Spirant must be supposed to have proceeded in the same direction, and in a parallel line, with that of the other Spirants, only perhaps more rapidly and much further. Among all of them the softening process clearly originated, as might be expected, in the interior or bivocalized position; which fact alone indicates distinctly enough the nature of the change (§ 34, last *note*, and Appendix G); but in the case of the Dentals the debilitation not only reduced under its sway all the Spirants so situated, but even attacked some of the initial ones.

(*k*) As the view I am defending represents beyond question the order of nature, there is no real need to enforce it further. Nevertheless, as Mr. Sweet's additional arguments raise one or two other points of interest, I may perhaps be allowed to suggest the way in which the theory of the foregoing treatise requires those points to be dealt with. As to the fact that H.G. *d* represents L.G. þ; it is a vital part of our theory that these two sounds were independent debilitations of the primeval Tenuis *t* (§§ 26, 31, 36, *et passim*); so that the strength of one of them gives no indication whatever of the strength of the other. Again: the fact that *dh* is written for *d* in some of the oldest remains of H.G. admits of two explanations: either *dh* was simply = *d*, just as, somewhat later, *th* was in H.G. (as it still is) = *t*; or, if *dh* really represented a Voiced Spirant, then that Spirant occupied precisely the same position between the ideally correct H.G. *d* and L.G. þ which *v* at the very same time occupied between the ideally correct H.G. *b* and L.G. *f* (§ 54 (*f*)); that is to say, it represents a partially executed *assimilation* of some of the H.G. subdialects to the L.G. in respect of the Dental as well as of the Labial Spirant; which movement, however, as regards

the Dental, was unable to advance, or even to maintain its ground. Once more: that the Latin digraph *th* should have been dropped or expelled from A-S. is just what we should expect; for, in the first place, there was already at least one native symbol (þ) (probably there was also a second (ð)) actually in possession. In the next place the A-S. Alphabet was based — not directly on the Roman Alphabet, but on the Old Irish variety of it[1]. The former, although used by the missionaries from the Continent, failed to establish even its own *simple* symbols (where these differed from the Irish), and was therefore still less likely to succeed with its *compound* ones. But, thirdly, the diagraph *th*, even on its native soil, never, so far as we know, represented a Spirant, and scarcely even the Greek form of the Aspirate (§ 35, *note*), except perhaps to the Roman *docti*, by or for whom it was invented. In all the Romance languages[2], as is well known, it is pronounced and mostly written as *t*; so that the combination was probably unsuitable for the Spirant, not so much on account of the hardness or energy, as of the too complete contact, assigned to it. From the fact that, in spite of all these drawbacks, attempts were actually made to employ the digraph at all, I should draw just the opposite conclusion to that which Mr. Sweet draws from the fact that the attempt was unsuccessful.

(*l*) The only point of interest now remaining to be considered is that arising out of the *shape* of the symbols þ and ð—or, really, as I shall presently show, of the older þ only. Mr. Sweet adopts the supposition[3] that this symbol was formed from the Latin D by producing the perpendicular stroke in both directions. Now D represents a soft sound, so therefore (for such is the assumption) did þ. But the danger of basing an argument on such an assumption will be manifest when we remember that the *wen* (p) approaches still more nearly to D in shape, the upright stroke being produced only *one* way. In point of fact, no inferences would generally be more fallacious than such as should propose to determine, from

[1] Latham, Introd. to Johnson's Dict., p. lxxiv; Earle, "Philol. of the Engl. Tongue," pp. 103, 109.

[2] Diez, Grammatik, i. 226.

[3] From Mr. Vigfússon, Icelandic Dict., *sub lit.* þ.

resemblances among symbols, either the relative phonetic powers of the sounds they stand for, or indeed the origin of the symbols themselves. On such grounds the powers of our O and Q, F and E, P and R should be nearly the same, and those of T and D, S and Z, F and V, K and G should be widely dissimilar; while it might be inferred that the latter member of each of the three first-named pairs of symbols was manufactured out of the former. But even where such an inference would be apparently correct it might be historically false—as in the case of P and B; for here it is certain that the former came—not from the latter (nor this from that), but—from the Greek Π by first shortening (Γ) and then bending up (Ꝑ) the right leg[1]. The only case in the Roman Alphabet and its descendants where the inference would be (under certain conditions) correct is that of C and G; of which the latter was originally a mere graphic variation of the former, although subsequently the former, oddly enough, acquired the power of, and at last ousted, the early Roman K. Yet this case, which (like others to be presently noticed) is one of spontaneous graphic radiation, as it were, within one and the same language, can hardly be quoted in support of the intentional manufacture of a new sign, for the purposes of one language, out of the symbols of a different language. Besides, the argument based on the supposed derivation of þ from D may be made to cut two ways. For the object of connecting those symbols is to suggest that the sounds they stand for have the common quality of *softness*. But the Soft sound represented, it is said, by þ became the parent of its corresponding Hard: why should it not be maintained therefore that the Soft Dental Mute was also the parent of its corresponding Hard? and so, of course, with respect to the Soft Guttural and Labial. Combining this extension with that in *h* (*ad fin.*) above, we should, in truth, give the doctrine in debate its widest scope; but, in so doing, we should both fight against patent facts, and overturn what have hitherto been considered the very foundations of Phonology.

(*m*) But if the supposed connexion of þ with *D* were as certain as it is uncertain, Mr. Sweet's main doctrine would

[1] See Ritschl, "Zur Gesch. des lat. Alphabets," in the "Rhein. Mus. für Philol.," 1869; or the first chapter of Corssen's "Aussprache."

not necessarily be any the more probable. For, as was just now said (paragraph (*f*) above), we cannot be sure that there were *not* two varieties of the Spirant at any given epoch, because there was, at the same epoch, but *one* sign, any more than we can assert that there *were* two varieties at a later period because there were then *two* signs. Let us for a moment suppose (what cannot be disproved) that *both* varieties of the Dental Spirant were in existence before the appropriation to that sound of the older sign; then, if only one sign was to be constructed for both varieties, and if that sign had to be based either on *t* or on *d*, the chances are clearly as much in favour of the selection of one of these letters as of the selection of the other; that is, it is just as likely that the Soft Spirant should have led to the selection of the Media, as that the Hard Spirant should have led to the selection of the Tenuis; so that the actual choice of *d* would, on this supposition, by no means involve the non-existence or juniority of the Hard Spirant. I believe, however, and indeed shall presently urge, both that there was, to a comparatively late period, only a single Dental Spirant, and also that the *two signs* (þ and ð) originally represented that *single sound*. Further, I should even be glad if the probabilities in favour of a connexion between þ and D were much stronger than they are; for I should then claim that connexion both as supporting and as being explained by the provisions of my own theory. But I should refer the connexion, not to a phonetic, but to a dialectic cause, and should look for its explanation in a state of things to which I have proposed to trace many other phenomena, viz., simple dialectic mixture. Where some of the L.G. tribes and sections of tribes used the correctly *verschoben* Spirant, others, in contact and even commingled with them, employed the Media (§§ 35 (*b*), 49 (*e*) *note, et alibi*), accompanying in this point the H.G. tribes, with which also many of them were, by our hypothesis, in contact (§ 53). Under these conditions an intimate relationship and approximate equivalence of the two sounds might instinctively have been taken for granted, and efforts might have been made to denote this relationship by constructing a symbol for the Spirant which should closely resemble the symbol for the Media. But any connexion between the

symbols is so doubtful that I feel precluded from basing any conclusion thereupon. If I were compelled to find an immediate parent for the symbol þ, I would rather turn to the Gothic symbol ƿ, which, when made swiftly by hand, would easily pass into the former by the gradual extension of one side of the curve at the expense of the other.

(*n*) The younger symbol (ð) may now be dismissed in few words. Its value as a piece of evidence depends entirely on the supposition that it was independently constructed out of *d*. Even then its value is small; for in its construction the precedent set by the older sign would naturally be followed. But I have little hesitation in totally rejecting that supposition. A life-long familiarity with the vagaries of handwriting convinces me (so far as the case allows of conviction) that the younger sign is not an independent creation at all, but simply a graphic variety of the older one, resulting from the gradual accumulation (perhaps the work of centuries) of minute differences of formation, of which the more marked stages may be represented thus:—

From this point of view the resemblance between *d* and ð becomes a curious accident. It is manifest, however, that while *d* (A-S. ƀ) does not, þ does, account for both the parts of ð. The cross line, in particular, which is such a striking feature of the latter, receives a perfectly rational explanation, instead of seeming to be a purely arbitrary addition[2]. Assuming, then, as we now may, that the sound, after the evolution of the younger symbol, was still felt to be a single one, we need not think it more remarkable that one old scribe should

[1] The extremes of this series differ much less than many other pairs and sets of symbols in daily use among ourselves, the members of which nevertheless are certainly derived one from another. Compare *C* and *G*, *R*, *r*, *z* and *s*, *ET* and &, *F* and *f*, &c.; compare also the quaint but elegant forms of the modern German script with their ancient prototypes.

[2] Compare the rudimentary cross stroke of the ordinary script *4* with the perpendicular stroke of 4, which it represents.

always write þ and another ð, than that one of our own correspondents should always write *ſ*, *z*, *ſs*, and another *ſ*, *r*, *ſſ*, and the like; nor yet more remarkable that in other old MSS. þ and ð are written indiscriminately, than that now-a-days *z* and *r*, *ſ* and *ſ*, &c. are, as often as not, used indiscriminately by one and the same writer[1]. What *is* remarkable is the way in which the language of signs instinctively follows in its methods the language of sounds. For just as (§§ 17, 28–30, and Appendix C) varieties of sound pair off with varieties of meaning, so (only more rarely) do varieties of symbol pair off with varieties of sound. Thus the letters *I* and *J*, as well as *U* and *V*, were, till quite recently, as Mr. Sweet reminds us, used (like the old Latin *C* and *G*) without distinction of value; and to each member of each of these pairs has been assigned a separate function almost in our own day. Precisely the same seems to have been the case with þ and ð. Used impartially and indiscriminately as long as the Dental Spirant was, or was felt to be, one, they gradually, after the younger sound was clearly marked off from the older, became assigned, the former generally to the Hard Spirant and the initial position, and the latter generally to the Soft Spirant and the interior position.

[1] Mr. Helfenstein (C. G., p. 126) seems to think the practice of the old scribes in this respect very reprehensible, and still more so that of those editors of their MSS. who imitate their "lawless" course. It is to be hoped, however, that editors will continue to be guilty of the same crime, and not virtually assume that the old copyists did not know what they were about. As to the point of phonetics discussed in this Appendix, it is manifest that such imaginary "lawlessness" is just what would give to any MS. its value as a piece of evidence; for it would mean, if it meant anything, that the distinction between the Soft and Hard Spirant either had not yet arisen or was not yet perceived, and also that there was attributed to the two symbols precisely the same phonetic value. The Teutonic scholars whose uniform distribution of þ to the initial and ð to the interior position (in conformity with the later state of our sounds) accords with Mr. Helfenstein's views, would have been, I imagine, the first to deride the notion that where their own fixed rule and practice was at variance with the phenomena presented by the remains of the older stages of our language, the latter ought to be adjusted to the former.

E.

On the possible Parent Forms of certain Related I-E. Roots.

(*a*) The close resemblance in meaning of certain simple *præ*vocalized roots (*e. g.*, *ak-*, *ag-*, *ap-*, *ar-*) to other simple *post*vocalized roots[1] exhibiting the same consonant (*e. g.*, *ka-*, *ga-*, *pa-*, *ra-*) has received considerable attention from those investigators who have endeavoured to penetrate to the ultimate elements of speech. Among these Dr. A. Fick holds a prominent place. The first *Nachwort* to his I-G. *Wörterbuch* is an elaborate and acute attempt to resolve all the biliteral (*i. e.* biconsonantal) roots, which were once regarded as the *primordia* of the I-E. languages, into uniliteral ones. The equivalence of these *præ-* and *post-*vocalized roots is there brought out with great force and distinctness; and while he confesses himself unable to account for the phenomenon, he says that an important step in philology will be gained by a satisfactory explanation of it.

(*b*) Unless a better explanation be forthcoming[2], I venture to suggest that one may perhaps be found in the proposition that each member of each pair of these apparently related roots is but a fragment of a still older *bi*vocalized root. The main and the common feature of such related roots is the consonant. In each member of the pair, however, we now hear but half of the full consonantal power[3]. In order to obtain the whole power we may consider the prævocalized root as complementary of the postvocalized root (or *vice versâ*), and the two together as making up one older bivocalized root. Thus *ak* and *ka* give an older *aka*; *at* and *ta*, *ata*; *ap* and *pa*, *apa*, &c.; so that, in the infancy of our parent language, uni-

[1] Richardson (Preface to Dictionary) applies to similar roots the terms "*an*nounced" and "*e*nounced." Perhaps the terms "concussive" and "explosive" indicate the nature of the roots as well as any.

[2] Schleicher treats the case as one of *Umstellung* or "transposition" (Comp., §§ 6 and 206); but this term, like many similar ones (§ 58 (*a*), *antè*), conveys no "explanation" of the phenomenon to which it is applied: no one can really imagine that vowels and consonants once played (so to say) at leap-frog after such a fashion.

[3] See Max Müller, Lectures, ii. 157.

literal roots would, on this view, have to be represented as severally exhibiting, if not a full, at least a rudimentary, vowel on each side; and we should have to suppose that after the organs of speech were put into position by a conscious effort, they were also relaxed by a conscious effort. In this there is nothing of *primâ facie* improbability. Our own familiarity with univocalized consonants proves nothing to the contrary; for this is merely a matter of education and habit, the tradition of which from each generation to its successor may have commenced in those hoary ages when the bivocalized root gradually underwent fissure into two parts.

(*c*) Now, granted that any ultimate roots were of this bivocalized form, the operative cause of their fissure is not far to seek. It would no doubt lie in dialectic varieties of *accent*. Mature and cultivated languages, indeed, are, as is well known, distinguished, each within itself, by a general uniformity and comparative stability of accentuation; but even closely related languages often exhibit, in comparison with one another, a discordance in this respect which may fairly be attributed to the development of older dialectic diversities. Thus the accent in Irish falls on the first syllable; in Welsh, on the penultimate. The same difference holds between Bohemian on the one side, and Polish on the other. Another case is supplied by the throwing back of the accent in Æolic and Latin as compared with the older Græco-Italian accentuation, which must have closely resembled that of Sanskrit[1]. With the exception, moreover, of the Sanskrit and the ancient Greek, whose general agreement points to a common primitive usage, the I-E. languages have worked out each its own independent system of accentuation, and no one system is now reducible to another[2]. We have already seen (Appendix C) how, in individual examples, even in our own language, a shifting of the accent has been laid hold of to indicate an incipient bifurcation of meaning; and we are now merely supposing that a similar movement set in at a period when it would be much more serviceable—in other words, that one of the differentiating forces of commingled dialects was at one time supplied by a tendency to a different accentuation, so that, instead of

[1] See Mr. Sayce's "Principles of Philology," pp. 336, 337.

[2] Schleicher, Comp., § 13, anm. 2.

an evenly balanced *aka, ata, apa,* one incipient dialect would at length pronounce *áka, áta, ápa,* and another *aká, atá, apá.* This would offer an excellent opportunity for doubling (if necessary) the resources of language within a considerable section of its vocabulary; and the duplicate form, when required, would be at once seized on by both or all the dialects for that purpose, in accordance with the principle described in §§ 28–30 and Appendix C. When these incipient varieties of form were once assigned to incipient varieties of meaning, their movement towards complete differentiation would be much accelerated, and would manifest itself in the further strengthening, in each case, of the accented at the expense of the unaccented syllable, which last would finally fade completely away[1].

(*d*) It need hardly be pointed out that this bivocalized form is the very one exhibited by many of the most venerable I-E. words, such as *agha-m*[2] = ἐγώ = *ego*; *adha* = ἔνθα; *ata* (Skt *atha*), "then"; *abha* = ἄμφω = *ambo*; *apa* = ἀπό; *ana* = ἀνά; and others. The fate of such words generally illustrates the principle which I set out to elaborate: they have all, sooner or later, passed from bivocalism to univocalism; and some have given off both a *præ-* and a *post*-vocalized form. Thus in ἐγώ = *ego* the *final* vowel evidently preponderates; and accordingly in the modern descendants of the Græco-Italian

[1] This Appendix, like one or two of the others, was originally written before the body of the foregoing treatise, and before I had noticed that Grimm (D.G., iii. 252) had made a similar suggestion in order to explain the relationship between certain pairs of prepositions; *e. g.*, Goth. *ana* = Ger. *an* = Engl. *on*, with which he in this way identifies the *na* of the Slavonic languages. Hence he surmises (p. 254) that Goth. *du*, Ger. *zu*, Engl. *to*, may be of the same origin as the Goth. and Engl. *at*, O.H.G. *az*, Lat. *ad*; which would imply a primitive *adu*. In his second "Philological Essay" Prof. Key proceeds to establish the existence of this primitive, and to show that we have both its parts in our *at* and *to*. These stray examples are of course covered by the wide generalization of the text above.

[2] It seems to me an excess of refinement to cut off the initial *a* of this particular root and of the corresponding plural (*asma*, "we"), and to make an independent "pronominal root" of it, as Fick, for example, does. By way of compensation, the consonantal part, which one would naturally take to be the very substance of the form, is then reduced to a mere "verstärkende Partikel."

languages the initial vowel has disappeared. In the Lithu-Germanic, however, the *initial* vowel must have preponderated from a very remote period (Goth. *ik*, Ger. *ich*, Lith. *asz*), and consequently the final vowel has disappeared. *Ata* gives off Lat. *et* and *at*; but *abha* gives off the Goth. *bai*, Ger. *bei-de* and our *bo-th*; and *apa* gives off Lat. *ab*, Goth. *af*, Engl. *of* and *of*. A remarkable example of such fissure in a modern language is furnished by the Latin *ille* (*illum*) and *illa* (*illam*), which in French have given off *il*, *elle*, &c. on the one side, and *le*, *la*, *les* on the other.

(*e*) In conclusion I will venture to indicate one other possible bearing of the suggestion just thrown out; and that is upon the question of so-called "prosthetic" (and perhaps also of "connecting") vowels. "Prosthesis," in particular, belongs to a large class of terms (See first note to (*b*) above) denoting arbitrary processes, whose intrusion into the realm of language should be viewed with the utmost suspicion. It would assist in warding off the intrusion to show that the assumed prosthetic vowels are, at least in some cases, the representatives or remains of the first of the pair of vowels above supposed to have once characterized some, if not all, roots. As an example of what is meant, let us take the stem of the oblique cases of the first personal pronoun singular. This is generally given in the form *ma* = "me." But here the Greek presents us with ἐμέ. Is the initial ε, then, only an arbitrary prefix, or a mere accretion, in the Greek? I am not sure of that. I should prefer to give a parent form *ama-*, from which the disappearance of the initial vowel is sufficiently explained by the accent on the final. Compare with this instance the closely related first person plural stem, which is generally given as *na-s*. But here the archaic Latin (Carm. Frat. Arv.) presents us with *enos*; and hence I should rather conjecture that *na-* is but the *post*vocalized part of a parent form *aná*, once perhaps identical with the Skt. *ana* = *ille* (in O.Lat. *olle*, *i. e. ollus*, for *on-u-lus*), which last form implies the *præ*vocalized part of the same root. Similarly ἐκεῖ-νος may imply, instead of *ki-* (=*ka-*, §§ 6 (*a*), 64 (*a*)), a primitive *aká-*; from which, by the hypothesis of this book, the first personal stem *agha-* was differentiated by debilitation of the Tenuis to the Aspirate (§ 31 (*c*)). Finally, the conjectures of this Ap-

pendix may perhaps help to explain some of the difficulties presented by connecting vowels, especially those coupled with verbal inflexions. Such vowels may be long, or short, or altogether wanting. Where we find a *long* vowel (*e.g.* in first person singular present, as *bhárāmi*, *φερω(μι)*, *fero*, originally *ferōm*, &c.), we may suppose the fusion of root and suffix to have taken place while the final element of the root still preserved its terminal vowel and the suffix its initial one (*e.g.*, *bhara-* + the aforesaid *ama*). In other instances (*e.g.*, the second and third person singular present) either the root or the suffix may have lost one of its vowels, so that the connecting vowel is *short*; and in yet other instances (*e.g.*, the perfect tense) both root and suffix may have lost their contiguous vowels before their final fusion; and hence we here meet with no connecting vowel at all.

F.

On Grassmann's Treatment of some Exceptions to Grimm's Law.

(*a*) The statistics of these Exceptions, as given by Lottner, have been somewhat modified by the powerful article of Grassmann already more than once referred to[1]. From many of the statements of that article it is impossible to differ; but with the application of its results (*in fine*) it is impossible to entirely agree. The author appears to claim for his own conclusions that they have abolished a large class of irregularities. But that claim requires important qualifications; for, even if all his own examples prove indubitable, he has merely *shifted* many of the irregularities from Σ_y to Σ_x[2], while he leaves quite out of the question the relationship of Σ_z to each of those systems, and of each of them to it. In pursuing his inquiry he brings out an important matter of fact which at first view seems hostile to the hypothesis of the foregoing treatise. My present business there-

[1] Kuhn's Z-S., xii. 80, &c.

[2] Nor does he hesitate to admit as much:—"Wenn ferner Got. *tek*-an dem Lat. *tang*-o, Griech. τε-ΤΑΓ-ων entspricht, so glaube ich, dass die unregelmässigkeit in den alten sprachen liegt" (p. 134).

fore is to suggest the line of explanation required to remove such apparent hostility. In treating of certain cases where **S** in Σ_y represents **S** in Σ_x (*e. g.*, Goth. *G*od-s = Grk *a*-Γ*aθ*-*ός*, or O.H.G. bi*B*-en = *φέ*B-*ομαι*), his contention, justly is that the German forms imply *bi*aspirate Cl. roots (*ghadh*- or *χaθ*-, *bhabh*- or *φεφ*-). But he contends further that such roots once *actually existed,* and that, at a later period, in virtue of certain euphonic rules, one of the two Aspirates became *de*aspirated. At this point, however, we must exercise caution; for the steps by which he supposes this deaspiration to have been effected are not altogether free from objection. Let us trace them upon one of his own examples as a typical case, viz. :—Skt. *budh*-nas = Grk *πυθ*-*μήν* = Lat. *fund*-us = O. Saxon *bod*-m. Of this series the Cl. parent is assumed to have been of the form *bhudh*-, both Aspirates being supposed *soft*. At some period subsequent to the separation of Greek from Sanskrit the "hardening" of the Aspirates in Greek (as maintained by Curtius) is supposed to have set in, and to have gradually run through the whole series of Aspirates; so that the root in question became, it is said, *phuth*- or *φυθ*-. In the next place Grk and Skt, though by this time long separated, proceeded, we are told, to deal with the root in precisely the same way; *i. e.*, they both deaspirated the initial, making in the one case *πυθ*-, and in the other *budh*-; while the Latin proceeded to deaspirate the final, and to thin down the initial into a spirant. Three doubtful points, therefore, present themselves at the outset :—first, the apparent disregard, or rather reversal, and that often repeated, of the Principle of Least Effort (*bh* and *dh* becoming *φ* and *θ* in Grk, *φ* becoming *π*, and *bh* in Skt becoming *b*); second, the multiplicity of forms supposed to have been assumed by the primitive Aspirate; and third, the accidental coincidence of action, on the one hand, on the part of two languages (Grk and Skt) long separated, contrasted with the diversity of action, on the other hand, on the part of two languages (Grk and Lat.) that remained much longer in contact. Upon the first and second of these points enough has already been said in the body of this treatise; as to the third, though it lies within the bounds of possibility, yet, if the commingled Cl. subdialects possessed bi-aspirate roots up to, and indeed long after, their Separation, one would

prefer to discover some distinct reason why they should all be so anxious, at a later period, to modify their ancient and native sounds.

(*b*) Now on the hypothesis of this book we need not believe that there *must*, but only that there *should*, have existed Cl. bi-aspirate roots, the *actual* existence of which may have been prevented by recognized causes[1]. Our hypothesis requires us to assume (§ 26), as the parent form of a family of words, a root in which only *Tenues* occur; and it demands that all the diverging forms should be derived therefrom by debilitation only. We are not permitted to suppose that primitive Tenues first descended to Soft Aspirates, and then remounted by successive stages to their original form as Tenues. In the example just considered, therefore, the parent root would be of the form $\{p + \text{vowel} + t\}$, *i. e.*, *pat-* or *put-*; and the two Tenues were, or should have been, preserved by the H.G. dialect, in which the normal form of the root should be *pot-*. The New H.G., indeed, in this case as in many others, exhibits, in *bod-en*, only the purely L.G. root *bod-*; but the O.H.G. exhibits both *bod*-am and *pod*-am. It is to be inferred that the latter, with the initial Tenuis, is the older. But the O.N. and A-S. exhibit the stem *bot-*, from which we may restore the final *t* of the root[2]. We are required therefore to take the primitive form *put-* (O.H.G. *pot-*) as the centre from which all the other forms radiated. The Cl. dialect, then, *should*, as was just now said, have produced, by the debilitation which in this example fell to its lot, the bi-aspirate root *phuth-* (*φυθ-*)[3]. But the Grk and Skt sub-

[1] I am here looking at this cluster of roots in the light in which Grassmann himself places them. In the note to § 49 (*e*) I have intentionally tried to show, with reference to the root *bhug-* (*φυγ-*), which is one of his examples, that a widely different explanation may, in that and perhaps other instances, be applicable.

[2] Unless it is forbidden to apply to European languages modes of reasoning which are legitimate as applied to the languages of Polynesia. See the method by which Max Müller (Lectures, ii. 138) restores *Savaiki* as an older form of *Hawaii*.

[3] I should fall back upon Grassmann's reiterated derivation of the primitive Hard Aspirate from the Tenuis (See Appendix B) as a valuable support to the mode of generation assumed in the present treatise, were it not that he makes that very Aspirate the parent in turn of the Tenuis in

dialects, at any rate, did not; and whatever may have been the cause of their objection to bi-aspirate roots, we must, by our hypothesis, suppose (what, in fact, is more likely) that it operated to *prevent the formation* of such roots, and not (what is less likely) to *alter* them after they had been in use for an unlimited time; that is to say, those subdialects (in accordance with the general principle laid down in §§ 48, 49) *maintained a Tenuis* which should have been debilitated to an Aspirate. This Tenuis therefore remained identical with a Tenuis in Σ_z, and (what was ultimately the same thing) corresponded to a Media in Σ_y (Scheme (ψ) of § 49 (*e*)). The resulting root (*puth-*, *πυθ-*) must then be supposed to have remained permanent in Greek (*πυθ-μήν*), while in Sanskrit the Aspirate, which was originally imperfect or was subsequently softened (§§ 33, 34, 45, *note*), ultimately, by assimilation, dragged down the initial Tenuis to a Media; in which movement it partially affected the still commingled Greek (*cf.* *βυθ-μός* and *βύσ-σος*, *i.e.*, *βυθ-jος*), so that both acquired, as regards the initial, a resemblance to Low German.

(*c*) The Latin subdialect, indeed, seems to have striven in its imperfect way to differentiate the root completely; and this fact, so long as it was assumed to be incontrovertible that all the Cl. dialects varied their roots from a common parent form, was an argument in favour of an original two-fold aspiration on the part of the other dialects. For on such an assumption the initial *f* in *f*undus necessarily implied an Aspirate in the once common form, while the internal *d* was quite consistent with the existence of the second Aspirate actually exhibited by Greek and Sanskrit. But it is provided by our hypothesis that the subdialects of a leading dialect, although agreeing in the main, may not in every case have carried out their differentiation to the same degree of perfection. We may suppose therefore that the Latin subdialect endeavoured to accompany the other Cl. subdialects in aspi-

German, Lithuanian, Slavonic, and Celtic. The Cl. Soft Aspirate, too, is supposed to beget both the Greek Hard Aspirate and also the L.G. Media, which latter again is supposed to beget the H.G. Tenuis. Thus we are here (as in the Schemes of Appendix D) in a whirligig, where everything becomes directly the parent of everything else, and indirectly of itself too.

rating (*more suo*) the primitive internal Tenuis; but that the resulting pseudo-aspirate speedily assumed the Medial form[1]. As the resulting root *pud-*, however, would still differ but slightly from the H.G. *put-* (*pot-*) and from the L.G. *bud-* (*bod-*), the Latin may further be supposed to have made an independent effort to dissimilate its initial Tenuis in conformity with the normal relationship between Σ_x, Σ_y, and Σ_z. Thus we arrive at the following dialectic series:—

H.G. . . . [*put-*], *pot-*;
L-G. . . . . [*put-*, *pot-*], *bod-*;
Greek . . [*put-*], $\pi\nu\theta$- and $\beta\nu\theta$-;
Sanskrit. . [*put-*], *puth-*, *pudh-*, *budh-*;
Latin . . . . [*put-*, *puth-*?], *pud-*, *phud-*, *fud-* and *fund-*;

where it will be seen that the history of the root in each of the Cl. dialects is now in strict harmony with the Principle of Least Effort. Under the H.G. its ideally correct form only is entered. The correct L.G. form was actually preserved by the Old Saxon, which, like the Latin, weakened both consonants; while the Old Norse and Anglo-Saxon, like the Greek, weakened only one. We are not, however, to suppose (what, indeed, is contrary to fact) that the O.N. and A-S. objected to roots of the form $\{\mathbf{S} + \text{vowel} + \mathbf{S}\}$, and that therefore, after evolving such roots, they "raised" one **S** to **H**. At this point the hypothesis of the foregoing treatise steps in with absolute impartiality; for just as it requires that the initial π of $\pi\nu\theta$- (instead of being supposed to pass through several changes with the absurd result of finding itself at last just where it was at first) should be considered as the primeval Tenuis preserved from any change at all, so it requires that in the more prevalent L.G. form *bot-* the final *t* should be similarly considered as another such unchanged primeval Tenuis. But after all there is considerable dialectic irregularity in this example[2]: where the L.G. dia-

[1] In conformity with the conjecture of Appendix G, the Media in the Latin root might be a direct debilitation from *t*; but until that conjecture has been discussed I leave the above paragraph as originally written.

[2] A more regular example (so far as it goes) is furnished by the root *pâk-*, "arm," "branch." This also was retained by the H.G., and is actually extant in the forms *puac*, *puoc*. The corresponding Cl. root

lects go astray (if they really do so), it is in quite a different direction from the Greek and Sanskrit.

(*f*) The force instigating to the violent changes assumed by Grassmann in the Cl. forms is supposed by him to have been euphony simply. But the hypothesis just substituted involves the *absence* of change. Hence the agency concerned must have been preventive rather than effective. Possibly the ultimate cause is to be found in our oft-repeated proposition, that so-called "substitution" was due to the contemporaneous and reciprocal action of commingled dialects. To say that dialects were commingled means, in other words, that the people speaking them remained in constant contact and communication. Hence it would manifestly be essential for one section, while yielding in general to the principle of Dissimilation, to resist it whenever there arose a necessity for preserving words from being deformed past recognition by other sections. We have (or fancied we had) already discovered the operation of a similar counteracting necessity in a less complex case of sound-change (§ 39 (*c*)), and that the necessity should have operated in a more complex case seems even more likely. We do not, indeed, find among the ancient dialects any striking objection to roots of the form $\{\mathbf{S}+\text{vowel}+\mathbf{S}\}$; but from all that we have conjectured respecting the earliest forms of the Aspirate, that sound may have seemed to the Cl. tribes to be a wider departure than the Media from the parent Tenuis; so that an instinctive objection to acquire roots of the form $\{\mathbf{A}+\text{vowel}+\mathbf{A}\}$ is, to say the least, not impossible.

G.

On the interior Mediæ for Aspiratæ in Latin.

(*a*) With reference to the subject of § 35, further thought emboldens me to hazard a conjecture which, when writing

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should have been of the form *phākh-* (*φᾱχ-*); but no trace of the initial Aspirate is discoverable (Skt *bâhu* = Zend *bâzu* = Gk *πῆχυ-*: the Latin is unfortunately wanting). The L.G. dialects preserve their correct forms: O.N. *bógr*, A-S. *bóg* (our *bough* and el*bow*), to which the M. and N.H.G. forms (*buog-* and *bug-*) are assimilated (§ 53).

that section, I was influenced by some opinions of previous writers to suppress. It depends immediately upon the main theory of this treatise, especially §§ 24–26, and amounts, in sum, to this—that previously to the spread of Aspiration through the Cl. dialects (§ 45) the Latin branch of the Italian subdialect might, independently of the other branches, have already so far yielded to the powerful influence of bivocalism (p. 72, *note* 2), or of conjoined liquids and nasals, as to have reduced to Mediæ *some* of those interior Tenues which in cognate Cl. dialects were still preserved as such, but which afterwards fell victims there to the new infection of Aspiration. Such softening has, it is true, been assigned (§ 35) to the H.G. dialect as its special characteristic, when viewed in relation to the symmetrical distribution of the mutes among the three great dialects; but it would be absurd to assert that so natural and universal a species of debilitation could not previously have prevailed sporadically, and perhaps over considerable areas outside those in which it was evoked by Dissimilation. In accordance with § 46 (*a*) any such interior Mediæ as our conjecture postulates would necessarily be protected from subsequent Aspiration; and, as to their number, there needed to be only just so many of them as might suffice to set the fashion, so to say, of correspondence with the interior Aspirates (or pseudo-aspirates, § 33) of the surrounding or commingled dialects and subdialects. Other interior Tenues would, by analogy (§ 21 *ad fin.*), generally adjust themselves to that fashion; and yet an exceptional instance or two would still be possible, in which, from special causes, an internal Tenuis might be drawn into the powerful Aspirating movement going on all around. Hence the *h* in *mihi, traho, veho.*

(*b*) It will, of course, be at once urged that any such tendency to softening ought not to have confined itself to those interior Tenues only which would otherwise have become Aspirates, but that it ought also to have affected some of those interior Tenues which should have remained Tenues. Probably it did so. At any rate, numerous individual examples of that form of debilitation are to be found in extant Latin. Thus (1) *g* occurs for interior *k* in *viginti, triginta, negotium, Saguntum, pagina, vagire, mugire,* and even initially in *guber-*

nator, gummi, gloriâ[1], and others; (2) *d* for interior *t* occurs only in the neighbourhood of liquids and nasals, as in *quadratus, mendax*, &c., or in terminations like *-idus*; but as a final in *quod, id, illud*, &c.; also in the whole series of archaic ablatives (*gnaivod, sententiad*, &c.) and archaic imperatives (*estod, facitod*, &c.); in all of which the primitive *t* was once, probably bivocalized (Appendix E)[2]; (3) *b* for *p*, both interior and initial, occurs in *ab* (primitive *apa* = ἀπό), *ob* (prim. *api* = ἐπί), *sub* (*s* + prim. *upa* = ὑπό), *bibo, buxus, Burrus* (in Ennius, for *Pyrrhus*), and others. A considerable number of instances of softening in proximity to liquids and nasals are omitted, although they really bear as forcibly on the point in debate as those that are quoted[3].

(*c*) It will further be urged that, if our conjecture were true, the interior *b*, which in Latin often represents the Cl. *Dental* Aspirate, ought to have originally appeared as *d*. But here, again, facts are so far in our favour, that several words belonging to the *same series* as the words in which that *b* occurs, actually do, and for aught we know always did, in Latin, as such, exhibit an interior *d*; as *medius, ædes, arduus, fundus, fides, vidua, abdere, credere*, and others. If, however, *all* the interior Mediæ answering to Dental Aspirates were once *d*'s, by what agency, it may be asked, could their transmutation into *b*'s have been effected? It will perhaps presently appear that even this difficulty is far from insuperable. I must first remark that, supposing the Cl. Aspirates to have been of the nature generally assigned to them (§ 12 and Appendix B), the difficulty just stated seems to me but microscopically greater (if greater at all) than that of getting from *dh* or *th* (= θ) to *bh* or *ph* (= φ); through which transition the interior *b* is, by many leading philologists (in conformity with Curtius's Theory of Aspiration (§ 12)), supposed to have been reached. The difference between the dental and labial seems to be, in each case, too wide and too sharply

[1] These and similar examples serve for an *à fortiori* argument.

[2] For cases of initial *d* = *t* I must refer the reader to Corssen, "Beiträge" &c., pp. 83 *et seqq.*

[3] For other examples of the way in which the tendency to softening continued to manifest itself throughout the whole history of the Latin tongue, see Corssen, "Aussprache," &c., pp. 77 *et seqq.*

marked to allow of an immediate passage from the former to the latter by way of spontaneous evolution [1].

(*d*) Leaving others, however, to bridge over the gap between *dh* and *bh* as they best can, I propose to remove our suggested change from *d* to *b* altogether out of the range of spontaneous evolution. As in the older stage of the movement (*a*, above), so here, I would fall back on the action of Analogical Adjustment evoked by the external pressure of dialects in contact (§ 21 and § 60 (*d*)). For the majority, probably the vast majority, of the Old Italians reduced their variety of the Dental Aspirate, in the interior as well as the initial position, at first, no doubt, to a series of Dental Spirants (=þ), and then to a series of *f*'s in no way distinguishable from the *f*'s derived from the *Labial* Aspirate. If we suppose this final phonetic stage to have been reached before the Italians loosened out into their subsequent divisions, the members of the rudimentary or potential Latin division,

[1] Corssen, Baudry, and others suppose the Cl. *Labial* Aspirate, among all the Italians, to have been first reduced to *f* in every position, and then, in Latin, closed up to *b* within words. Their views as to the Dental and Guttural Aspirates are not easy to make out. Ascoli, in a series of vigorous articles (Kuhn's Z-S., xvii. and xviii.), lays down a scheme of evolution that certainly has the merit of consistency, although the consistency appertains to the most doubtful point; for he would reduce *all* the Aspirates to Spirants in all positions, and the Dental Spirant (þ) in some cases to *f*, and then evolve from them the internal Mediæ of Latin. Now there can be no doubt that the initial Spirants of Latin, and the initial and interior Spirants of the other Italian dialects, descended immediately from the Italian variety of the Cl. Aspirate (§ 33, *suprà*); also, that where *f* represents the *Dental* Aspirate it must have passed through the stage represented by þ; for these two sounds are formed in close proximity, and the actual transition from þ to *f* is demonstrably frequent. But for the closing up of *h*, þ, and *f* into *g*, *d*, and *b*, I should like to see some further evidence than the mere fact that, in Latin, Mediæ appear where Spirants appear in the non-Latin dialects. (Ascoli's illustrations from the Portuguese—as *abrego* = *africus*—fall under § 60 (*d*) *suprà*; those from the Gothic have, on our hypothesis, no bearing on the case.) If the Latin went with these other dialects as far as Ascoli supposes, we might have expected it, instead of diverging to a different line of evolution, to have gone on in the same line, as did some others of the said dialects; *e. g.*, the Umbrian, which even labialized *s*. Thus *aprufe* = Lat. *apros*; *avif*=*aves*; *sif*=*sues*; *tuf* or *duf*=*duos*. (Schleicher, Comp., pp. 102, 266.) Here we must suppose that the *s* first became þ (a change common among lispers in this country), and then that þ became *f* as before.

among whom the interior *d* still maintained its place, would gradually come to experience an uneasy sensation—to acquire a more or less dim perception—that their own internal Mediæ were not in continuous harmony with the single and uniform series of *f*'s of the bulk of their fellow-Italians. Where, indeed, the latter said *tife, ife, sife, pufe, Safinims, alfa, Alfius,* or the like, they themselves said *tibei, ibei, sibei, quobei, Sabiniôm, alba, Albius,* &c. &c.; and thus, in a large number of instances, perpetuated the correct and regular correspondence originally established; but in a smaller number of cases, as where the latter now said *mefia, rufro, ufer, verfim, prufa-fam, -fas, -fat, -fans,*[1] or the like, (for the older *meþia, ruþro,* &c.,) they themselves continued to say *media, rudro, uder, verdum, proba-dam, -das, -dat, -dant*[2], &c. &c.; and here, although they were really in the right, (a fact, however, for many long ages forgotten,) a feeling to the opposite effect would be evoked by the preponderating influence of the dialects in contact. It is therefore just what might be expected, that they should proceed to adjust the latter series of examples to the pattern set by the former, and thus endeavour to establish that continuous phonetic harmony which they felt to be wanting: that is to say, these internal *d*'s would, at first occasionally and irregularly, and then permanently, be in this way transformed one by one directly to *b*'s[3]. But if we suppose that, before the permanent transformation of the whole series of *d*'s was completed, any sufficient cause, such as the loosening out of the Italians into detached or almost detached divisions, arrested (as it certainly would) the influence of the other dialects upon the Latin, then we ought to find a part of

[1] These forms are mostly conjectural, except as regards the *f*.

[2] I adopt the keen-eyed view of Scherer ("Zur Gesch." &c., p. 202), which connects the Lat. imperfect suffix with the Cl. root *dha* = L.G. *da* = our "do", and thus links on the Latin (as regards the evolution of this grammatical form) to the German and Lithuanian dialects. (See Schleicher, Comp., p. 795.)

[3] It would be quite consistent with the above hypothesis that the transmutation of the Latin *d*'s to *b*'s should have followed close upon, and have advanced *pari passu* (so far as it did advance) with, the descent of the non-Latin þ's to *f*'s. In this case the extant Latin *d*'s coincide with a part of the series of non-Latin þ's, which had not yet descended to *f*'s when the dialects separated.

that series still remaining as *d*'s; and this is exactly what we *do* find. (See the examples in *c*, above.)

(*e*) I shall not now stay to elaborate the foregoing outline of a hypothesis. It will be obvious, however, that our fundamental conjecture (*a*, above) may easily be shown to have an important bearing in many other directions than that in which it has been here applied—as, for example, upon the origin of similar Mediæ in Greek, in Li-Sl., and in various L.G. dialects. In reference to the latter two especially it might suggest some modifications of, or additions to, the suggestions thrown out in §§ 46, 49; and I regret that advantage was not there taken of it. Phonetically, its claims to consideration are:—(1) that it reduces the total amount of sound-change to a minimum; (2) that all the changes are brought under the Principle of Least Effort; and (3) that, where the line of phonetic descent is oblique instead of direct (*i. e.*, from *d* to *b*), it points out an agency potent enough to wrench the line aside.

ADDENDA ET CORRIGENDA.

Page 11, line 11. Of the four symbols **H, S.—A, A'**, the first three are wanting in some copies, having dropped out at press.

20, *note, add*: "So also are our numerous constructions with *of* (id., pp. 481-2)."

26–28. The general fact here established was noticed by Lord Bacon 250 years ago:—"Linguae quae ex *Gothis* fluxere *aspiratis gaudent*" (De Aug. Sci. vi. 1); but, of course, his "Goths" do not include the Mœso-Goths. (I am indebted for this interesting reference to Dr. Farrar's "Families of Speech.")

27, line 5 of *note* 2. After "*would*," understand: "on the Historical Hypothesis."

40, last line but 6, and page 48, line 9. The statement as to the exact equivalence of the interchanged sounds should be so far qualified as to admit of subsequent natural debilitation in such sounds after the separation of one dialect from the other. (See Appendix D (*e*).)

41, line 12, *for* II.G. *read* L.G.

47, line 12. With reference to the "fact of some interest" a friend remarks, "I should think everybody has noticed this."

51, *note* 5. It had escaped me for a moment that Max Müller himself, in another place (Lectures, ii. 137), demonstrates very forcibly the present corrupted condition of modern Hawaian.

56, line 14 of *note*. So, too, thinks M. Hovelacque in his recently published work "La Linguistique" (Introd.):—"Nous ne cherchercns pas à éviter l'examen de la question de l'origine du langage. .... L'écarter sous prétext equ'il faut proscrire toute recherche des 'origines premières,' c'est admettre la possibilité même de ces causes premières, dont les mathématiques et la chimie ont fait justice." But I do not see how H.'s transference of the question to anthropology (or rather anthropoidology) can possibly yield any satisfactory results.

58, line 10. The "trebling" of expressive capacity, here spoken of, must be referred to monoliteral roots, or, rather, roots originally involving *a single tenuis* (*e. g.*, *kar* on p. 65). In the case of roots which were originally of the form {*tenuis*+*vowel*+*tenuis*} the *possible* increase of capacity would manifestly be *ninefold*; for each variety of initial (tenuis, media, and aspirata) *might* be combined with each variety of final.

61, line 2 of formula (ξ), the last D should be D'.

64, line 1 of § 31, **S** is wanting in some copies.

Page 71, line 14, "rightly enough," &c. I mean (as § 33 showed) that Hard Aspirates may have existed in one dialect of the Holethnos and Soft Aspirates in another.

72, § 35; and pages 95, 96. In reference to the questions here treated, Appendix G should be read.

73, *note*. A short but valuable illustration of the subject-matter of this note has recently appeared (where philological information would hardly be looked for) in the Blue-book for 1875-6, issued by the Committee of Council on Education. A paragraph in Mr. Rhys's Report on the Schools of Denbighshire and Flintshire gives some interesting examples of the contortions that English sounds undergo in Welsh mouths. See also an excellent article on that Report in the "Saturday Review" of Sept. 16, 1876.

75, *note* 2. The internal *h* of *mihi* should have been spoken of as "solitary" among the Latin representatives of the *Labial* Aspirate only, a qualification which was inadvertently omitted. Internal *h* represents a *Guttural* Aspirate in "*trahere*" and "*vehere*." (See Appendix G (*a*)). In line 4 of same *note*, for "Curtius," substitute: "Schleicher, Comp., p. 243."

78, with *note*, and page 92. So M. Hovelacque:—"Nous pouvons penser, sans crainte d'erreur, qu'avant leurs migrations, ces populations occupaient un territoire assez vaste. En ces larges limites la langue commune indo-européenne ne devait-elle point se modifier, s'altérer, se corrompre de façon différente dans les différentes tribus établies sur ce territoire? Nous pensons qu'il n'en pouvait être autrement."—"La Linguistique," p. 339. See also the excellent fourth and fifth Lectures of Prof. Whitney.

80, subsection (*b*). The general similarity of linguistic conditions would lead us to expect that the interchange here treated of might be discovered, at least on a small scale, in the popular dialect of Normandy. Accordingly, "Le *v* devient *w* assez fréquemment: 'je m'en *w*ais' (*v*ais) 'Cha*w*ois' (nom propre, Chauvois)," &c. See "Histoire et Glossaire du Normand," par Ed. Le Héricher, vol. i. p. 32. Where German words that once began with *w* are preserved, the *w* has generally become *v*, as *v*atre, *v*inche, = *w*ater, *w*inch. (Id., iii. pp. 51, 52 of Appendix.) I have not been able to discover any trace of such interchange in the Norman-French transplanted to this country.

97, *note*. In line 2, *read* "Sprache*in*heit;" and in line 9, *for* Salv. *read* Slav.

106, *note*, bottom line, after *-ths*, *add*: "which, however, is not really an exception (See Schleicher, Comp., p. 316)."

114, line 6, after "reduplication," *add*: "on the pattern of *pa-paver*, *cu-cumis*, *cin-cinnus*, *su-surrus*, &c."; line 7, "Aedic" is a misprint for "Æolic."

118, par. (v.), line 13, after "*feder*," *add*: "and on the pattern of our *wiggle-waggle*, *tittle-tattle*, *riff-raff*, &c."

Page 121, line 10, *for* bhaksjam *read* bhaksh*j*am.

127, last line but 2, *read*: "(in *apparent* contradiction, &c.)." *High* and *Low* are really, of course, geographical epithets corresponding to *Upper* and *Lower* as applied to provinces.

134, line 22, *for* absorbed *read* supplanted,—the superimposed dialect having really migrated.

144, line 2 of 59 (*a*). The reference ([1]) to the foot-note has dropped out.

147, line 3 of first *note*, after "Grammar" *supply* "§ 218, note 2."

153 & 154, subsection (*e*) and *notes*, and 174 (*f*). The following is from the article in the "Saturday Review" of Sept. 16, 1876, referred to in the Addendum to p. 73, *note*, above:—"The [English] short *i* is often turned into the North-Welsh *u*, which is nearly the same as the German *ü*. Here a sound is brought back, though perhaps not in its right place, which has dropped out of standard English, though it still lives in Devonshire and East Anglia. The general tendency of language is to change the *ü* into *i*. Our old English *y* was doubtless *ü* . . . . The Greek *υ*, which is now indistinguishable from *i*, was doubtless *ü*." This would justify my alternative proposal to refer the Aryan combination *ky*, as well as (or instead of) the European *kw*, to an older *kü*.

155, lines 8 and 9. It is not impossible that some of the sibilated *k*'s may have been thus debilitated subsequently to the Separation, having previously belonged to the *pure k*'s common to both the great dialects (See *note* to § 63 (*c*)).

184, lines 1–10, and page 185, *note*. I am very much inclined to suspect that M. Hovelacque's reference of the (apparent) changes of the mutes by the Germans to a general *renforcement* all round must be due to some such mistaken estimate of the phonetic value of the Spirants—or, at least, of the Dental Spirants—as is adverted to in these places. ("La Linguistique," pp. 284 *et seqq.*) He decides, apparently on the strength of some expiring provincialisms, that our Spirants are on the road to, and will ultimately become, Mediæ. In those days the Lord's Prayer (if it has not disappeared with our *religiosité*) is to begin, "Our Bader gwich art in gebben." (We shall fortunately, by that time, have been long dead.) But if the Spirants were originally due to a *renforcement* of the Tenues, their passage to Mediæ must be a terrible *affaiblissement*; so that, from his own point of view, H.'s line of phonetic change takes opposite directions in different parts of its course.

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